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AND
HISTORY

An Essay on
Problems of Co-ordination

by 11469
F. T. WAINWRIGHT

with a Foreword by
SIR FRANK STENTON



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Maps 1 and 2 are reproduced, by permission, from H. C. Darby: *An Historical Geography of England before A.D. 1800* (Cambridge University Press). Map 1 is based upon the names listed in E. Ekwall: *English Place-Names in -ing* (Lund, 1923) and Map 2 on the Ordnance Survey 'Map of Britain in the Dark Ages (South Sheet) 1935'.

Maps 4 and 5 are reproduced from chapters by K. H. Jackson and R. B. K. Stevenson in *The Problem of the Picts* edited by F. T. Wainwright (Thos. Nelson & Sons Ltd).

FOREWORD

by Sir Frank Stenton

FREDERICK WAINWRIGHT, who died after a brief illness on 12 June 1961, at the age of 43, was a prominent figure among the modern scholars who are bringing a variety of technical disciplines to bear on the problems of British history in the Dark Ages. He knew at first hand the way in which place-names can be used as materials for history, he was acute at discerning the fragments of genuine tradition which may lurk in late historical compilations, and he was a first-rate field archaeologist. Avoiding the dangerous enterprise of large-scale historical reconstruction, for which in his field the time is not yet ripe, he has left his mark on the studies to which he devoted his life through a series of articles already passing into the body of knowledge on which historians can rely. His early work on the reign of Edward the Elder, resting in the main on narrative sources, produced results which no future historian of this difficult period can safely ignore. As time went on, his archaeological interests became more prominent. In a study of the souterrains of Southern Pictland, now ready for publication, he gives new precision to our knowledge of an important, but superficially unattractive group of monuments. The study is based on his own excavations, where he showed himself an archaeologist of the modern type, for whom discoveries of

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material are of secondary importance in comparison with the establishment of plans and sections. The same qualities appear in other excavations which he carried out at Chirbury, Criclade, and Tamworth, Anglo-Saxon fortresses of the tenth century, where the main object was to obtain indications of an original plan. Sites like these are generally infertile in datable objects, but the Tamworth excavation produced a cut halfpenny of Æthelred II struck at Torksey, a discovery interesting historically, and also as an example of a type of find which gives particular satisfaction to the modern numismatist but rarely comes his way. In these and other excavations Wainwright showed himself an admirable director of voluntary assistants, by whom his enthusiasm on the ground will never be forgotten. Here, and in all fields through which his studies ran, he was in constant touch with other workers, and in his later years, the number of those who came within his circle was very large. He was Honorary Treasurer of The English Place-Name Society, and had collected much material for a volume on the place-names of Leicestershire and Rutland. He started and was General Editor of the important series of *Studies in History and Archaeology* now being carried on by Messrs. Routledge & Kegan Paul. Wainwright contributed two essays to the first volume, on the Problem of the Picts, and left two others ready for publication in a forthcoming volume on the Northern Isles. In the present book, which is printed as it left his hand, he sets out his reflections on the principles which had governed his conception of historical investigation throughout his working life, and in particular on the necessity of aiming continuously at the co-ordination of historical, linguistic and archaeological lines of inquiry. He was well aware of the difficulties which

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beset this method of approach, and indeed emphasizes them repeatedly in this book. They do not affect the value of the method as an ideal. Some of his readers who had the good fortune to know him personally may come to feel that the strongest argument in its favour is the large volume of firmly based research which it enabled him to carry through within the tragically brief term of years at his command.

FRANK M. STENTON

My part in the publication of this book has been to prepare the text for press and to correct the proofs. My husband and I discussed the book page by page as it was written, and I have made no attempt to edit his work.

BARBARA M. WAINWRIGHT

31 January 1962

PREFACE

For a long time I have wanted to write a book about the difficulties and problems that arise in attempts to co-ordinate historical, archaeological and linguistic evidence, but for an equally long time I have known that I shall never write the book I have in mind. It should be a work of considerable length and detailed criticism, and it should be a serious excursion into philosophy, a subject in which I can claim no competence. I have written short notes about the problems of co-ordination from time to time, as in *Antiquity* XXIII (1949), and I have discussed them occasionally in lectures, as at the Anglo-American Conference of Historians in 1957, but I have always been reluctant, I think wisely, to attempt in print more than a few paragraphs or a short review.

As I shall never write the book I should like to write, perhaps I may be permitted to explain how I came to write the present book. It was in the nature of an accident. Soon after my address to the Anglo-American Conference of Historians in 1957 Dr. H. P. R. Finberg kindly invited me to contribute a chapter on similar lines to his forthcoming *Approaches to History*. I resisted the invitation, as I had resisted others, for I knew that the spoken word would not in this instance submit to the compression and orderliness of the printed page. But at last, for one reason or another, I agreed to make the effort and in January 1961

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I began to write what was intended to be a chapter in Dr. Finberg's volume. As I had feared, it fell far short of what I felt the subject demanded, and yet by the beginning of April it had run to some 35,000 words. This was too long for a chapter in Dr. Finberg's volume, but it seemed that it might serve as an introductory volume to a new series on *Studies in History, Archaeology and Language* which Messrs. Routledge and Kegan Paul were about to launch under my general editorship. And this is what it has become.

It began as an essay, not as a book, and to a great extent it retains its original character. It was already more than three-quarters written before the decision to turn it into a book was finally taken. The only thing to do, short of rewriting it completely, was to go through the typescript, change the words 'section' to 'chapter' and 'chapter' to 'book' wherever they occurred, try to think up appropriate headings for the new divisions and let it go at that. This explanation is intended as an apology rather than as an excuse for any faults of arrangement and balance.

Notwithstanding my reluctance, still strong, to commit myself to print on a subject that requires deep thought and careful writing, I hope that my comments, strung together under a single theme, may be useful to students, especially to students at the beginning of their studies who may find it convenient to have in their hands a book which draws attention to some of the problems they are likely to meet and may provide a basis for discussion. I have worked in the three fields of history, archaeology and place-name studies, and the problems that I raise are ever present in my mind. I should not claim, however, to have solved them to my own satisfaction.

It will be noticed that there is an almost complete ab-

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sence of footnote documentation and reference to authorities in this book. This springs from the original design for a short essay, of course, but I think it is not out of place in a somewhat fuller survey. I am, after all, discussing ideas rather than the results of research and it matters little who has said what, especially as I am more concerned to set down what I think myself than to contradict the expressed opinions of others. The latter as a rule are introduced only to emphasize a point or illustrate a comparison.

Not that I would be thought to disregard my very great indebtedness to scholars past and present. I have been fortunate in having studied history under the great master of our time, and it was he who also guided my first steps in place-name studies. I think I may say, too, that for many years I have known and counted among my friends the leading archaeologists and place-name scholars in Britain and on the Continent. From their conversations no less than from their published works I have probably drawn everything of value in the present essay. To saddle them with my own views by naming them might be thought a poor reward for all that I have gained from them.

4 April 1961

F. T. WAINWRIGHT

I

INTRODUCTION

THE studies of archaeology and language offer much to the historian, much that he could never derive or infer from the study of historical sources alone. This is true of all periods of history, but it is more obviously true of the period between the departure of the Romans and the arrival of the Normans than it is of any other period in the history of Britain. In these dark centuries the historian frequently finds himself struggling with intractable sources, suspended in a frustration of doubt and uncertainty, and reaching out for solid ground from the confusing shadow of ambiguities and apparent contradictions. The contributions of language and archaeology have come to be regarded as indispensable to him.

This is not to say that linguistic and archaeological studies exist only to serve the historian. They exist in their own right, of course, with objectives and methods proper to themselves. They are not mere ancillary aids to the study of history. And it is not to say that other studies and other disciplines have no contribution to make to the

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study of history. Of course they have. But in this book the discussion is confined to archaeology and place-names and to the nature of their contribution to the history of Britain in the six centuries between the middle of the fifth and the middle of the eleventh. This limitation is imposed only by the design of the book, for its object is to consider the main problems of co-ordination in so far as they concern history, archaeology and place-names. Examples are usually taken from the Dark Ages in Britain, but it need not be emphasized that they have a wider potential application.

The historian, the archaeologist and the student of language can co-operate in all periods for which there is historical evidence, archaeological remains and linguistic information. In the prehistoric periods proper the archaeologist is undisputed master: he may seek assistance from other scholars, chiefly from natural and physical scientists, but the historian has no place where there is no documentary record and the philologist can offer only frivolous guesses where languages are quite unknown. Archaeology is not synonymous with prehistory, however, and not all archaeologists are exclusively prehistorians. The archaeological approach to archaeological material has an important contribution to make to the study of all periods of history, even modern history. If this is sometimes overlooked, it is largely because the abundance of historical evidence for, say, the nineteenth century leaves a comparatively smaller field for the student of material remains. It is also at least partly because most archaeologists in Britain devote themselves to prehistory, with the result that mediaeval archaeology is a recent growth and modern archaeology is still largely in the hands of collectors, dealers and *littérateurs*. Even for recent centuries, how-

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ever, the importance of the archaeological contribution is now widely recognized; and indeed it is difficult to see how the Industrial Revolution, to take but one example, can be fully understood without the examination of surviving structures and without the institution of technological inquiries of the kind that are inherent in the archaeological approach. The study of language, too, has its part to play in the study of all periods, though for the study of recent centuries it is likely to remain marginal by reason of the abundance of other kinds of evidence. In the period under review, however, there is no abundance of documentary evidence, and so the contributions of archaeology and language are correspondingly magnified in importance. They rank as equal partners with history proper in the building up of a reliable picture of the past.

To preach the value of collaboration today would be to preach an accepted gospel. Without the contributions of archaeology and place-names the story of the Anglo-Saxon settlements in Britain would be incomparably less complete and incomparably less accurate, the complexities of the Pictish north would be less understood, and the significance of the Scandinavian impact on the British Isles would scarcely be comprehended at all. In these and many other fields the collaboration of historians, archaeologists and place-name scholars has been fruitful beyond the dreams of students of an earlier generation. Neither archaeology nor place-names can supply the precise historical narrative of events, the names of kings and leaders, the interaction of cause and effect, the motives, the explanations and the aspirations of men, the formal details of social organizations, comments on the character of political authority and comments on the process of political change—all of which, under ideal circumstances, may

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emerge from trustworthy historical sources. But in this period, where documents are few and often not very informative, archaeology and place-names greatly amplify the historical record, not infrequently mitigating its deficiencies and repairing its omissions.

From archaeology we may expect information about a people's economy and way of life, their houses and their villages, their temples and their churches, their crops, animals and food supply, the extent of their control over natural resources, their implements, their weapons and their fortifications, their crafts and technological processes, their contacts with other peoples, certain aspects of their trade, their burial customs, their artistic achievements and the general level of their material culture. Place-names supply in full measure linguistic information of a kind that is absent in archaeology and usually ignored or blurred in the historical record. They also supply fairly precise conclusions on the intensity of settlements, linguistic boundaries, origins and relationships, with occasional comments on social and economic conditions. This information is primarily linguistic, of course, but its political and social implications are often less ambiguous and more convincing than the equivalent implications inherent in archaeological material. In the world of economies and technologies, however, archaeology has a far greater contribution to offer than either place-names or the historical record.

These studies have greatly extended the potential frontiers of our knowledge. History, archaeology and the study of place-names each provide information that is badly preserved or not preserved at all in the other two. They are to a great extent complementary to each other, and whenever two or more of them have been brought

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together in effective collaboration our understanding of the problems at issue has always been widened and deepened. The spectator who casts a casual eye towards this period may well imagine that the stage is set for great achievements and that the principal performers are already advancing confidently into a future full of promise and free from doubt. In this instance the spectator does not see much of the game.

Promise there is, and that in great measure, but beneath the surface there are strains and stresses. All students of this period are engaged in the struggle to uncover hidden facts and facets, all are seeking a fuller and more accurate picture of the past, and all are to an increasing extent dependent on each other. But they do not always present a united front. Each has his own problems, each is apt to misunderstand the contributions of the others, and, as a result, unresolved differences of opinion are not uncommon in this field.

The three main stages of advance, though often overlapping in practice, may be defined thus: first there is the recognition and collection of the evidence; secondly there is its interpretation in terms consistent with itself and within the limits of its essential character; and thirdly there is the co-ordination and integration of results into a single synthesis. Sometimes this single synthesis is described, by historians as well as by archaeologists and place-name scholars, as a 'historical synthesis', in which sense the term history is used to cover all aspects of and all forms of inquiry into the human past. This usage, increasingly common among archaeologists, is unobjectionable so long as it involves no confusion of thought, but the historian's history is essentially the study of the past as it is revealed by the written record, and in this

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book the word history is generally so used unless a clear indication of a wider or narrower connotation is given.

Difficulties and complications arise in each of the three stages of advance, and most of all in the third stage, for the co-ordination of results from different approaches is a singularly intricate proceeding, the hazards of which even today are not sufficiently recognized. The basic reason for these difficulties lies in the fact that the materials for the study of history, archaeology and place-names respectively are inherently different in character, and from these differences in character arise different methods and techniques and, above all, different kinds of conclusions. There is plenty of scope for misunderstanding and disagreement, and no one who works in this rewarding field can afford to ignore or forget the essentially different characteristics of history, archaeology and place-names.

The materials proper to the study of history are written or documentary. Within their range are included inscriptions on coins and stones, codes of law, documents noting the existence or transfer of property, rights and obligations, public records and private memoranda of all kinds, and deliberately conceived narrative and expository works such as chronicles, annalistic compilations, sagas, treatises, biographies and compositions in which artistic presentation takes precedence over reality of content. A library catalogue is historical evidence and so, too, is an attempt in writing to expose the basic truths of human existence. Some historical materials are intended to convey information, some to entertain only, and some to deceive. Some are aimed at contemporaries, some are aimed at posterity and some are not aimed at all. Some are meant for the eyes of many and some are meant only for the eyes

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of the author or the recipient. Here at once arise questions of truth and trustworthiness.

A third definition of history, narrower than the two referred to above and supported by dictionaries and etymologists, would limit historical materials to those that present a consecutive record or narrative of events. It is not the definition adopted in this book, but it is a legitimate definition and it is helpful when the nature of evidence is under discussion. Even those who reject it in practice, however, often urge that the most trustworthy historical materials are those of the 'account-book' kind—on the grounds that, as their purpose is usually to record rather than to narrate, explain or convince, they are less likely to be biased or misleading. An archaeologist might be tempted to say that they are 'unconscious' evidence in the sense that they convey information unintended by those who composed them, but the word 'unconscious' is both inaccurate and deceptive when applied to historical materials and, for that matter, even more inappropriate when applied to archaeological materials (see below, p. 45). In any case, 'account-book' materials, even the most formal of documents, may be misleading, deliberately composed to support an argument or prove a point rather than to record a fact, and of course they often are. One need only mention the fabrication of Anglo-Saxon charters.

When the historian turns from 'account-book' materials to works of narrative and exposition his difficulties increase. It is true that these sources provide him with much fuller information of the kind he is seeking—sequences, causes, explanations, motives and aspirations—but the greater scope and detail of the information are offset by greater problems of reliability. Consider the

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problems of assessing the trustworthiness of information offered by a chronicle and of comparing it with, say, the trustworthiness of information provided by a saga or a saint's life. A statement in any of these sources may be true or false, an accurate summary by someone who knew the facts, an ill-formed assumption by someone who did not, an official or censored version, a piece of propaganda, an imaginative fiction, or even a deliberate attempt to deceive. More will be said of these problems below. The point here is that the written evidence of history, unlike the material evidence of archaeology and the linguistic evidence of place-names, is direct evidence only of the 'state of mind' of the person who composed it or controlled the composition of it. It is often fuller, more precise and more revealing than the evidence of archaeology and place-names, but its trustworthiness is incomparably more difficult to assess.

The archaeologist has no equivalent problem. His piece of pottery is a piece of pottery, and the question of true or false does not arise. All archaeological evidence is material evidence, the material evidence left behind as the result of man's activities on earth. It includes pieces of pottery, fragments of swords, jewelled ornaments of high intrinsic value, worthless and discarded implements, huts and palaces, physical traces of man's disturbance of the soil, and indeed all material remains from pins to hill-forts. It includes, of course, the stone on which the inscription is carved and the vellum on which the chronicle is written. Its essential characteristic is that it is material. It is visible, tangible, and of itself straightforward. The archaeologist may not know what an object is, or he may fail to interpret it correctly, but the evidence itself cannot lie and cannot deceive. It may contain oddities like toy

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implements, minlature weapons and, in modern times, explosive cigars or oranges of soap, but such objects are not deccits as archaeological evidence, for archaeological examination at once reveals their character (which is not the same as the purpose for which they were devised). They are not like forgeries or misleading statements in historical sources, for these do not necessarily reveal their true character by their composition. Historical evidence, as explained above, is direct evidence only of a state of mind. Archacological evidence, by contrast, is direct evidence of practical skills, technological processes, aesthetle interests and physical sequences. This distinction is of fundamental importance. Compared with historical evidence, archaeological evidence is simple and uncomplicated; but, as will be seen, special techniques are required for its recovery and the legitimate inferences that may be drawn from it are limited.

The evidence of place-names falls into quite another category. Place-names are part of language, and the student of language is concerned primarily with sounds. He cannot pick up and handle a vowel; he cannot see it; he cannot even hear it as it was. Except in so far as current speech and pronunciation can be studied by the worker in the field, or nowadays mechanically recorded for the future, he deals with written forms or representations of sounds as they occur in historical and archaeological contexts. For the student of place-names these are the older spellings of the names he is seeking to elucidate, and they come before him not as sounds but as attempts to represent them in writing. Some attempts are less successful than others, a fact which explains why there are so many distorted or freak forms among the older spellings of place-names. In this respect, therefore, the philologist

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is at a disadvantage compared with the archaeologist and the historian, but, as will be seen, he has advantages over both in that he can more confidently summon to his assistance a body of general rules and comparative material.

It should never be forgotten that the fundamental characteristic of place-names is that they are linguistic phenomena. The older spellings, upon which the place-name scholar rightly sets such store, may be compared to the material remains of the archaeologist and to the documentary evidence of the historian, but they are not material like the remains of archaeology and they cannot be described as true or false like the written evidence of history. As representations of spoken sounds they may be described in terms of accuracy or distortion. That is all. But it is an important distinction. Place-names arise from, are influenced by and throw light upon the language and speech habits of people. By their nature the only direct information they can supply is linguistic information. It is true that they may be used to provide information of a non-linguistic character, information that is often of very considerable interest to the historian, but this can only be by implication or assumption, never by direct inference.

Since place-names arise from and are influenced by living languages they are, of course, subject to the rules that govern the development of languages, but in at least one respect they stand apart from other elements in living languages. As they are names attached to places, not to objects, they quickly become identificatory labels rather than descriptive words, and so they tend to lose their original meanings for those who use them. Which is why the great majority of place-names in Britain now have no meaning in terms of ordinary words for the millions of

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people who daily use them. Ordinary words fall out of use when they cease to be meaningful, but place-names remain in use for centuries after they have become meaningless as words, for they still effectively perform their basic function of identification. They are not exempt from ordinary phonological changes—this should be emphasized—but they are in some ways more and in some ways less open to certain influences which affect meaningful words. On the one hand they show a tendency to preserve archaic forms not now present in the living language; on the other hand they are often more influenced by analogy, especially 'popular etymology', the effect of which is to change their forms by cutting across ordinary phonological developments and by assimilating them to forms which may be more readily understood. The philologist who is also a place-name scholar must keep such considerations in mind. And all who seek to use place-names for non-linguistic purposes must be aware of them.

The above survey of the nature of the evidence available to historians, archaeologists and place-names scholars respectively is no more than a summary, but it will have served its purpose if it has emphasized the essential differences which distinguish each of the three kinds of evidence from the other two. Problems of interpretation arise chiefly from the nature of the evidence, but it is from these differences that most of the problems of co-ordination arise.

II

HISTORICAL EVIDENCE

BEFORE turning to problems peculiar to the historian, a few more words may be written about terminology. Historians are not especially given to misusing terms or to using the wrong terms, and in this respect they are usually more careful than archaeologists, but, however careful they are, there is ample scope for confusion. Even the word history itself carries many meanings. Three have been noted above: it may be used to cover all aspects of the human past (in which all-embracing sense it is extended to cover not only prehistory but sometimes also, as in the term natural history, accounts of non-human phenomena), it may be used to cover the study of the past as it is revealed in written records (in which sense it is generally, though not exclusively, used in this book), and it may be limited to the study of the past in so far as it is based upon narrative and coherent records of events (in which sense it excludes many written documents and, incidentally, leads to such distinctions as that between chronicles and annals). In another sense the emphasis is

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on reliability, and the word is used, against words like myth and fable, to represent fact as distinct from fiction. And in yet another sense it is used to cover what purports to be true irrespectively of whether or not it is. When we say that a certain figure is historical, do we mean that beyond all doubt he once had a human existence on earth, or do we mean that the evidence for this belief is preserved in written records, or do we mean that it comes from a trustworthy source as distinct from a myth or a legend, or do we mean that, even though it comes from an untrustworthy source, it is nevertheless acceptable as true? Conversely, when we say that a certain figure is unhistorical, do we mean that he is a figment of the imagination, or do we mean that he is never mentioned in written sources, or do we mean that his existence is in doubt because he is never mentioned in trustworthy sources, or do we mean that, though he is mentioned in trustworthy sources, the evidence itself is unreliable and therefore unacceptable? And how do we regard traditions which, though handed down by word of mouth for centuries, appear in the written record only at a comparatively late date?

It is not suggested that any one of the above interpretations of history is correct, to the exclusion of others, or that all cannot properly be used in their own contexts. The English language was not devised as a vehicle for the arguments of scholars, and we must accept the fact that many words have several meanings. In this book it will be found that words like material, form and element are used in different senses, and sometimes in senses which at first glance appear to contradict each other. The point is not so much that we should avoid apparent inconsistencies but that at least we should be quite sure what we

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mean and, above all, nor build any argument on what we do not mean. It is more difficult than it would seem, and few scholars would claim that they never nod.

The problems that the historian has to face in the examination and interpretation of his material have been foreshadowed to some extent in the preceding pages, and in any case this is not the place for a full discussion of them. His immediate problem is to assess the trustworthiness of his sources. This is often a difficult and complicated business. The chronicle before him, for example, is probably not the original version; it may exist only in copies or in copies of copies of the original—with mistakes, additions and amendments introduced at each stage—and the so-called original itself may have been a composite work. Consider the complexity of the Anglo-Saxon Chronicle. The basic stock or common archetype, compiled towards the end of the ninth century, was a composite work, largely copied from other sources, some known, some unknown, with much in it of a traditional nature, with much that is artificial at least in arrangement, and with only a small part of it strictly original (i.e. not taken from an earlier written source). And this archetype no longer exists. Instead we have copies of various dates, copies not of the archetype itself but derived from it through other copies. The existing copies, and copies which are known or may be inferred to have existed, represent several different versions of or deviations from the common stock, each with its own divergences, omissions, additions and continuations. None of them are original except in respect of those entries added to a particular manuscript and not copied from another written source. Before we quote an entry in any particular manuscript we ought to know how it came to be there,

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whether or not it represents a contemporary statement, and if it comes from an artificial reconstruction of traditional material or from Bede or from another set of northern annals or from annals compiled in Mercia, and so on. And then we must examine the trustworthiness of the source from which it came.

Investigations devised to answer such questions must be undertaken before any entry in the Anglo-Saxon Chronicle can be used as a reliable and meaningful historical statement. It is no longer permissible to quote from the Anglo-Saxon Chronicle as though it were a unitary work, and the day of the conflate translation has surely gone for ever. Exactly one hundred years ago (1861) an excellent edition of the Chronicle was published, with the texts of its various manuscripts arranged in parallel columns; and in a separate volume was published a conflate translation. The editor thought it necessary to apologize for the arrangement of his texts because of 'the great mass of repetition', but he did not think it necessary to defend his translation. The texts are still useful, despite amendments inevitably made necessary by the researches of a hundred years; the translation has long been useless to serious scholars. Yet fifty years after its publication a distinguished historian, whose prestige later brought him a knighthood, possessed only the useless translation and essayed to write a history of the Anglo-Saxon period without paying anything like adequate attention to the texts themselves.¹ What a difference marks the approach

¹ This criticism requires the support of some evidence, not least because anonymity will not here hide identity. About twenty-five years ago the present writer bought a copy of the 1861 volume that contained the texts of the Anglo-Saxon Chronicle and, without an eye to the future financial value of his purchase, neglected to buy the companion volume of translation. Later he bought other copies of the 1861 edition, always in pairs

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to his source of another distinguished and knightly historian whose work is today the standard history of the Anglo-Saxon period! It is difficult to believe that both works belong to the same half-century.

The Anglo-Saxon Chronicle provides but one set of examples of the historian's problems. Every other source provides its own, and they are all different. An Irish compilation now called (incorrectly) the Three Fragments of Irish Annals (*Fragmenta tria Annalium Hiberniae*) survives, unlike the Anglo-Saxon Chronicle, in a single manuscript only; it is a copy of a copy made in 1643 by Dubháitach Mac Fir-Bhisigh (DuaId MacFirbis) from a manuscript which cannot now be traced. Therefore it carries no immediate conviction of antiquity or trustworthiness—rather the reverse, for in arrangement it is confused, in content it has a legendary character, and in style it bears the marks of an artificial literary composition. Despite its unprepossessing character, at least one section of it is based upon contemporary written accounts of the ninth

as issued, but for years the solitary volume of texts stood alone to remind him of the kind of book-buying error that may spring from enthusiasm and ignorance. At last, however, a bookseller advertized a copy of the translation alone, and the opportunity to rectify the youthful error was seized. The newly acquired copy came from the library of the late distinguished and knightly historian. In a moment of curiosity and to check what was already a suspicion, the present writer addressed an inquiry to the bookseller and received the following reply: '. . . We have all the books from the library of the late Sir ——— and we are able to state that he did not possess a copy of the first volume of Thorpe's *Anglo-Saxon Chronicle*.' This is not conclusive proof—the volume of texts may have been lost or given away—but it is at least significant. There is no doubt that the deceased historian knew the volume of texts, of course, just as the present author knew the volume of translation when he did not buy it, but that he did not pay anything like adequate attention to it is amply demonstrated by his published history of the Anglo-Saxon period.

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and tenth centuries, a conclusion which is supported by independent and often oblique corroboration in the annals of England and Wales and by the survival, amid much legendary matter, of archaic forms of words and names. The compilation known as the Annals of Ulster is another example, more coherent and more widely known, of how early and contemporary written accounts can survive across the centuries to appear in manuscripts of much later date. It is often held that the mediaeval chroniclers have little or no contribution to make to the study of Anglo-Saxon England; this may be true as a general statement, but often they incorporate into their works contemporary records of a much earlier date and in this way occasionally they preserve trustworthy information that otherwise would have been lost.

It is the historian's task to recognize and isolate such trustworthy material. But each chronicle, each manuscript for that matter, is unique. The place-name scholar is usually able to check the reliability of his material by reference to general phonological rules of wide applicability. Not so the unfortunate historian. There is no body of rules to assist him in disentangling the development of his source. Each source must be disentangled and assessed separately. And all this must be done before trustworthy historical information can be extracted from it.

Codes of law and charters must be treated with equal circumspection. Several Anglo-Saxon codes of law have come down to us in copies and, by and large, their contents may be accepted as trustworthy; but when we consider what reasons lay behind the copying of earlier codes, often out of date, it is apparent that we must reckon with the possibility of editorial and other slight modifications

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and with the possibility that the extant copies do not exactly represent the texts of the codes as they were issued. The study of charters presents a more treacherous field. The immediate test of authenticity depends upon the absence of suspicious features in the historical content, the linguistic form, the formulæ, the dating clause and the list of witnesses; but interpolations, omissions and modifications as well as accidental errors often occur in copies of genuine charters, and even a forged charter is not proof of the non-existence of the grant which it purports to record. One must remember that charters could be fabricated to support a truth as well as to support an untruth. And even the most blatant forgeries, with their central statements as untrue as their settings are suspicious, may embody trustworthy material derived from genuine charters or trustworthy incidental information derived from other sources. The complexities of this approach are sufficiently obvious, and it need only be said that the trustworthiness of historical information is not strictly dependent on the trustworthiness of the setting in which it is found.

When the historian has determined the date, provenance and ultimate source of his materials he still has to face the task of assessing the reliability of the information it offers. It is not enough to have ascertained that the information occurs in or is derived from a contemporary source. He must ask himself if the author had access to the facts, if he was competent to summarize them accurately, and if he was trying to do this or trying to mislead. In charters, codes of law and similar material the information given is usually incidental, and to that extent it may be less likely to mislead (except in some cases of forgery), but also as a rule it is less complete and less explicit than the informa-

tion offered by narrative and expository sources. With codes of law it may be clear that they represent intentions but it is not always clear that they closely represent existing social and economic conditions, with charters it is especially unwise to argue from their silence on certain points, and neither offer the explicit comments and explanations of a good narrative source. But the more complete and more explicit sources, which set out to inform or explain, are correspondingly more difficult to assess, for they are more closely dependent on the state of mind of the author. A statement in a chronicle is not direct evidence of its truth but only of the author's state of mind, and it may be remarked that even a belief that a proposition is true is only indirect and often not very convincing evidence of its truth.

Since the historian is dealing with material which is direct evidence only of a state of mind it follows that he can never achieve certainty or proof in the mathematical sense, and it is often difficult enough for him to achieve what he would regard as historical certainty or proof. To many specific events he is able to accord a degree of probability sufficient to entitle them to be accepted as historical facts, but he is on far more difficult ground when it comes to assessing the aims and motives said to lie behind them and when it comes to assessing their significance in a wider historical pattern. There is a world of difference between, say, a statement in Egils Saga and one in the Anglo-Saxon Chronicle. Or between any one chronicle or manuscript and another. Or between Bede's *Historia Ecclesiastica Gentis Anglorum* and Adamnan's *Vita Sancti Columbae*. Or, for that matter, between Bede's measured opinion of Columba and that of Adamnan. In comparing Bede's statements on Columba with those of

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Adamnan the historian must take into account not only access to reliable information, competence and intention but also attitudes, prejudices, conceptions and all those influences which combine to give each author his own particular state of mind towards Columba. And Columba's character was such that it has inspired many different states of mind towards him through the ages.

In making his assessment the historian must be careful not to apply inappropriate standards. It would be foolish, for example, to expect hagiography to conform to the standards of modern critical biography. Lives of saints contain stories that are true, stories that might be true, stories that could have been true and stories that could not have been true; they also contain stories unashamedly borrowed from the lives of other saints, sometimes whole passages thus transplanted, and of course they contain miracles by the dozen. Even in the case of miracles it is not necessary to assume that hagiographers were misled by coincidences or unexplained natural phenomena. They could fairly claim to be revealing a deeper truth in that the apostles and their successors had received a promise from Christ that they would perform miracles, and to doubt that saints could and did perform miracles would be to doubt the word of God. It is a question of how one conceives truth. Simply because a historian today cannot accept some of these stories as true he ought not to dismiss their authors as dishonest or deluded men and he certainly ought not to extend his distrust of them to other aspects of their work. When it comes to recording and explaining incidents of human affairs such authors are no more likely to mislead us than many modern scientists.

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The critical appreciation of historical sources is of comparatively recent growth. Few early writers approached their evidence with the scholarly caution of Bede, and until modern times even the most distinguished scholars had almost unbounded confidence in the written record, without regard to the circumstances under which it had been compiled and transmitted. Even today euhemerism and rationalization, common faults among folklorists, are not altogether abandoned by some historians, and figures of myth and legend are sometimes accepted as historical. It is not always easy to distinguish sharply between fact and fiction, and the early history of the Celtic peoples with its sophisticated blend of truth and imagination is an especially treacherous morass.

Exactly how the historian approaches his task—by the application of external and internal checks of various kinds and by the more subtle methods of *Quellenkritik*—need not concern us here. When he has satisfied himself of the nature and validity of his evidence it still remains for him to integrate it into an already existing pattern and to draw his own conclusions. This requires qualities of judgement and a broad human experience, and it also requires a set of values. We may compare Adamnan's values with those current in an age which lays increasing emphasis on material and technological progress, and we may reflect on the warning that our own set of values is neither final nor absolute.

Enough has been said of the historian's difficulties. Perhaps few philologists fully appreciate them, many archaeologists certainly do not, and it is fair to say that not all historians are fully aware of them. The philologist often detects the historian's apparently specious arguments, and the archaeologist not infrequently disregards,

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to his own downfall, the complexity of the historian's sources. All are dealing with human materials, but the materials of the archaeologist and the philologist are far less closely associated with a state of mind.

III

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IT has been emphasized above (pp. 8-9) that the archaeologist is primarily concerned with the material remains of the past, not with the written record or the evidence of language. As a general rule, therefore, he is not called upon to face problems and difficulties of the kind that confront the historian. He has problems and difficulties of his own, of course, and that in no small measure, but they arise less from the need to test the validity of his evidence than from the need to find and interpret it.

The recognition, recovery and recording of primary archaeological material involves the application of more or less specialized techniques. Perhaps the simplest is the careful observing and accurate recording of archaeological monuments as they are visible on the ground today, though their identification and interpretation is far from a simple operation and it can proceed successfully only from a wide background of archaeological knowledge and experience. Observation from the air, usually with its

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accompanying record of photographs taken at different heights and from different angles, reveals much that cannot be seen or comprehended by an observer on the ground. And tentative explorations below the surface of the ground can be carried out by electrical resistivity surveys and by other less complicated techniques. But none of these approaches can compare in importance with the techniques of excavation, which are devoted to the recovery and recording of archaeological information in its proper context and under conditions of scientific control.

It need hardly be emphasized today that the aim of excavation is not, as it used to be, simply to produce works of art and other objects for museums or wealthy patrons. Ideally an excavation is a deliberate and planned piece of research designed to provide answers to certain specific questions. Even on the numerous occasions when an excavation is carried out as an emergency operation before a threat of destruction the excavator is obliged to ask specific questions of his site. The questions must be capable of being answered by the evidence, and the techniques employed must be capable of extracting the answers from it. An excavator does not—or should not—dig with only vague questions in his mind or simply to see what fate will hand out to him. If he does, he will end up with vague answers or no answers or with mere pieces for a museum. He gives much thought to his questions and to the best means of extracting the answers from his site, and therefore he pays great attention to the layout and organization of his excavation and to the selection and employment of his techniques.

His aim is to find out exactly what has happened in a material sense at his site. According to its nature and to

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the content of his questions he must decide where and when to concentrate on stratigraphy in order to understand sequences and where and when to switch from 'vertical digging' to 'horizontal digging' in order to obtain plans and a fuller picture of the material aspects of a particular phase in the occupation. His aim throughout is to obtain and to record accurately as complete a picture as possible in the light of questions formulated in advance and added to or modified as the work proceeds. He will call to his assistance an increasing number of scientists—geologists and petrologists, botanists, palaeobotanists and pollen-analysts, chemical analysts, spectrographic analysts, zoologists, soil scientists, men who can measure the disintegration of radioactive carbon, and other specialists—for these may be able to throw light on the age, nature and environment of his site and on the objects which it produces. The objects themselves are primarily pieces of evidence which by their context may help to give an approximate date to his site or to some sequence in its development or by their nature may provide information about the practical skills, technological processes, aesthetic interests and cultural contacts of the people who used them. Other inferences may be made from them, as will be seen, but only in the last resort do they become mere objects for display in a museum.

Here we can no more discuss in detail the archaeologist's techniques than we could discuss in detail the historian's techniques for testing the trustworthiness of historical sources; and of the techniques of excavation it must suffice to say that they are 'the most important and the most complex of all forms of archaeological investigation'. They could be explained only in a much larger work or against the background of specific excavations, but

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they must be understood by anyone who aspires to make use of archaeological evidence. This applies as much to the non-excavating archaeologist who wants to use archaeological evidence for archaeological purposes as to the historian who wants to use it for non-archaeological purposes or to integrate it into a non-archaeological synthesis. As for the scientists who are willing to assist him, the archaeologist must at least understand the potentialities and limitations of their techniques, or he will not be able to take full advantage of their assistance or to assess the relevance of their results to his own questions.

In addition to archaeological material obtained from and observed during excavations there are also the objects now in museums and other archaeological collections, i.e. objects from earlier excavations and objects from chance discoveries made under conditions inadequately recorded or now forgotten and therefore unknown. There is a vast accumulation of such material stored in repositories all over the world, and it is primary archaeological evidence in that it is direct evidence of the practical skills, technological processes and aesthetic interests of the people who made or used it. There can be no doubt of its importance or of the importance of the results that may be obtained from the study of it. But from the archaeologist's point of view it is mutilated evidence, for it is divorced from its proper archaeological context and a great part of the information once preserved with it was lost when it was removed from the ground. It is no longer able to throw direct light on the physical sequences and structural evolution of the sites from which it came. It is, incidentally, a mute reminder to all excavators that their work inevitably involves the destruction of evidence.

Reports on excavations and descriptions of objects

divorced from their archaeological contexts—these include descriptions of objects appended to excavation reports as well as descriptions of objects in museums and other collections—are sometimes called secondary archaeological evidence. If they seem to have the character of historical evidence, that is not surprising, for they are historical evidence, though of a rather specialized kind. They are not archaeological evidence at all. They pose the same questions of trustworthiness as other historical sources, though fortunately in less complicated forms as a rule. An author's state of mind is involved, and therefore the reliability of all these written accounts is open to debate and suspicion in a way that archaeological evidence proper can never be. Questions of honesty and competence arise. Of the former nothing need be said, and of the latter it has been pointedly observed that the reliability of evidence derived from an excavation 'is in direct proportion to the competence and skill of the excavator'. One archaeologist has admitted in print that he keeps a mental list of excavators on whose reports he can rely, and everyone who tries to use excavation reports must do the same. It is a comparatively simple matter to make assessments of this kind if one knows an excavator personally or watches him at work, but the same questions of reliability must be raised against all excavation reports whether they come from contemporaries or from an earlier generation. One must apply to them much the same tests as a historian applies to his sources, using whatever internal or external checks present themselves, taking into account other works by the same author, and even bearing in mind that an obsession or blind spot may or may not colour the whole of a report—just as a belief in miracles may, or may not, bring the whole work of a

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hagiographer into disrepute. Even the most incompetent and unpromising reports contain reliable evidence of one kind or another, though its isolation and interpretation may be a task for the trained and skilful critic. And it is at this point that the specialized character of excavation reports becomes apparent. From time to time non-archaeologists of high critical ability have used or summarized excavation reports with conspicuous success, but it remains a fact that fully competent criticism is possible only by archaeologists who themselves have first-hand experience of the problems, techniques and difficulties of excavation.

Incompetent excavation is a sore point with most archaeologists. No permanent harm is done if untrustworthy descriptions are published of objects still accessible in museums, for the faults in them can be corrected by re-examination; but excavation always destroys evidence, and the losses arising from incompetent excavation are irreparable. This explains much of the anger and abuse that rend archaeological circles from time to time and horrify other scholars by their violence. It would be unreasonable, however, to expect archaeologists to react to the wanton destruction of their evidence with less violence than historians would react if one of their fellows carelessly burnt a manuscript after trying to read it. Especially if the fellow made a habit of such careless destruction, as some excavators seem to do. Every excavation involves the destruction of evidence. No one is infallible, no one is perfect, and much evidence will always evade even the most skilful and the most conscientious excavator, if only because it lies outside his mental vision or the capabilities of his techniques. It is all the more important, therefore, that an excavator should spare no effort to get

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his questions right, keep them right, and use the best available techniques with all the precision that he can command. All serious excavators regard excavation as one of the heaviest of responsibilities. Which probably explains and may excuse their occasional outbursts of anger against thoughtless visitors, incompetent colleagues and critics who do not understand their difficulties.

When we come to consider problems of interpretation there is one that faces the archaeologist in a way that its equivalent does not face the historian. An excavator is compelled to make tentative interpretations of his evidence while the collecting of it is still in progress, that is during the process of excavation. He cannot as a rule go back and re-examine it, and unless he makes tentative interpretations as he goes along he will not be able to modify his questions or frame new ones to meet his changing appreciation of the potentialities of his site. When his evidence has been dug away he cannot re-examine it like the historian can have another look at his manuscript and reframe his questions at leisure. By a complicated system of controls, commonly in the form of standing baulks, he can often come near to having another look at what has gone, but even the best of these systems has obvious limitations. And in many cases it is quite impossible for him to make all the necessary interpretations as his work proceeds, if only because these often require specialists' reports which may not be available until months after an excavation has ended.

Any attempt to interpret archaeological evidence is at once controlled by the limitations of the techniques and conditions under which it was collected. These will vary from excavation to excavation, from age to age, and from excavator to excavator. Ignorance of them or

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failure to comprehend them fully will undermine the validity of any interpretation. Historians who know nothing about excavation will find themselves on dangerous ground if they venture into this field of interpretation in the hope of incorporating archaeological evidence into some broader non-archaeological synthesis. And one could wish that more archaeologists stood on firm ground in this matter.

It is the nature of archaeological evidence, however, that imposes the strictest limitations on its interpretation. Its character has been summarized above (pp. 8-9), and it may be repeated here that it is direct evidence only of practical skills, technological processes, aesthetic interests and physical sequences. It can tell us how a pot, a weapon or a hill-fort was made, what materials were used and how they were put together; sometimes it can tell us something about the aesthetic appreciation of the maker, owner or user; and often from its context in the ground it can tell us if it was laid down—constructed in the case of a hill-fort but only deposited in the case of a pot or weapon—before or after another pot, weapon or hill-fort. Some purists among archaeologists, especially those who claim that archaeology is a science, would call a halt to interpretation at this stage, and it must be admitted that the direct inferences that can properly be drawn from archaeological material have been more or less exhausted.

It is both possible and legitimate to advance to further inferences so long as it is understood that they are not direct inferences and that they depend to some extent upon assumptions. The presence, nature, quantity and context of animal bones, for example, and the existence of agricultural implements, querns and even traces of cereals, as

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well as the character of surviving structures and the physical layout of a settlement, all permit inferences concerning the economic bases and the economic arrangements of a community. Population densities may be estimated, standards of material prosperity may be compared, and even cultural and trading contacts may be inferred. It could be argued that economic information of this kind constitutes archaeology's most important contribution to our knowledge of the past, and in general terms there can be little doubt about its reliability. But it should be remembered that it is to some extent based upon assumptions and that its reliability in any particular instance, therefore, is dependent upon the validity of the assumptions involved. It should be remembered, too, that it never presents a complete picture: much relevant archaeological material has been lost (for only durable remains survive in the ground), and of course not all aspects of an economy find expression in material remains. Finally, it should be noted that some archaeological remains which today seem to have had economic implications may have had an entirely non-economic significance for the people who used them. Many economic interpretations must have been offered for objects and structures which, if the truth were known, we should see were primarily or wholly sociological or mystical in significance. Invalid assumptions can lead us very far from the truth.

To advance still further by inferences and to seek sociological and political information from archaeological evidence is a very hazardous proceeding. The fact of the matter, whether the archaeologist likes it or not, is that these aspects of human life seldom find expression in material remains. If an attempt is made to force evidence to reveal what it cannot be expected to reveal, assump-

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tions loom larger and inferences become correspondingly strained. Political allegiances and obligations, formalities of land tenure, details of social structures, arrangements for the preservation of law and order and similar aspects of human activity are usually quite beyond the range of the archaeologist and archaeological evidence. Material remains may reflect these aspects, it is true, but as a rule the reflection is too faint and too ambiguous to form a reasonable basis for inference. Burial customs and the nature of grave-goods, for example, may reflect social classes, but how can we be sure that this is the true explanation of the observed material distinctions? A territorial distribution of certain archaeological features may represent a political unity underlying a cultural unity, but how can we be sure of this from archaeological evidence alone? A few broad generalizations are sometimes permissible, and tentative conclusions may gain support from non-archaeological evidence, especially from historical evidence or linguistic distributions, but as a rule unsupported archaeological evidence can supply only a few hints and clues which add up to no more than a series of unresolved and unresolvable possibilities. Interesting and often relevant though some of these may be to other scholars, the archaeologist as such would be wise to recognize that there is not necessarily a determinable logical relationship between certain aspects of human activity and the material remains that are the proper study of archaeology.

Inferential excursions are sometimes made into a fourth field of human activity, the field of human thought and aspiration. This is even more remote from the material evidence of archaeology, and inferences are correspondingly more hazardous. So hazardous that in most cases

they can be written off as either platitudinous generalities or illogical absurdities. In short there seems to be no way of obtaining reliable information about men's thoughts from the material objects which they have left behind. The mere existence of tombs and temples may be taken to imply some sort of belief in immortality or the supernatural, and from their construction and other physical clues it is possible to make inferences about ceremonies that attend, for example, the disposal of the dead. But we delude ourselves if we believe that the physical features of tombs and temples take us any distance along the road towards understanding the thoughts, emotions, convictions and aspirations that lie behind them. It is true that, in this field as in others, archaeological evidence can often illustrate or clarify evidence from non-archaeological sources and so make its own definite contribution to a fuller understanding of these most elusive aspects of human endeavour. But—and this is the point—of itself it is incapable of producing reliable conclusions as distinct from incomplete collections of indiscriminate possibilities. A recently departed and very distinguished archaeologist, who himself had made many attempts to penetrate the minds and reveal the thoughts of early peoples, expressed his conclusions in a valedictory message received by colleagues after his death: 'I believe foredoomed to failure any attempt to recapture the subjective motives or emotions that inspired the overt acts the results of which alone survive in the archaeological record. Archaeologists must resign themselves to adopting a behaviourist position as much as students of animal psychology.'

In addition to the limitations imposed on inference by the nature of archaeological evidence and by the conditions under which it has been recovered, it should be

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remembered that the evidence can never be complete. Flesh, wood, bark, fibres, wool, furs, skins and leather are among the materials that usually decay and disappear, and in some soils bones, teeth and even metals are destroyed. It is true that skilful excavation supplemented by inference can recover traces of some of this lost material, wooden buildings from post-holes for example, but on many sites the life of a community is represented only by objects of stone, metal and pottery and by the physical layout of its structures. And in many cases whole sites have been swept away by natural agencies such as wind or encroaching seas and by human agencies such as quarrying or other industrial operations. What is left is only a fraction of the potential archaeological record, the fraction that has escaped destruction by what archaeologists call the accident of survival. Much of it still lies buried in the ground and some of it will come to light, if ever it does, only by a combination of the chance that it will be uncovered, the chance that it will be noticed and the chance that it will be reported. Therefore we must allow for the accident of discovery as well as for the accident of survival. Archaeologists, and historians too, usually appreciate the limitation that this incompleteness imposes on their inferences, but it is sometimes overlooked, especially by scientists who, thinking of controlled experiments in a laboratory or of data fed into a machine, occasionally interpret archaeological evidence on the faulty assumption that what is before them is all that ever existed. Archaeologists and historians are aware that they are in possession of only a small proportion of the evidence that once existed and that their results must be correspondingly tentative. They cannot use the evidence they do not have, but they must not forget about it.

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The basis of reliable interpretation is the asking of the right questions, that is questions capable of being answered from the evidence available, and all questions addressed to archaeological evidence must take into account its limitations. Otherwise they will be illogical and absurd. There are many logical and legitimate questions that one can ask of a broch, for example, but it is no use asking if the broch-builders were Picts, if all brochs were built by the same people, if they believed in angels or if they were Celts. Answers to such questions could be supplied archaeologically by a single site only by the discovery of something like a tablet bearing the statement that all brochs were built by Picts, that they spoke a Celtic language and that they believed in angels. And such a statement would be historical evidence, not archaeological evidence, and it would have to be subjected to rigid tests of trustworthiness before it could be accepted as true. The attribution of the famous Alfred Jewel to King Alfred depends hardly at all upon archaeological evidence, for this can do no more than answer questions about its discovery, composition, manufacture, decoration, purpose and date. The attribution depends almost entirely on the historical statement in open-work lettering round its edge—ÆLFRED MEC HEHT GEWYRCAN—and, taken alone, this statement does not go very far towards proving that the Alfred named on the jewel is Alfred son of Æthelwulf and King of the West Saxons. Support for or comment on such a conclusion must come from arguments based on other evidence, archaeological, historical and linguistic. The limitations imposed on inference by the nature of archaeological evidence are severe, but they must be strictly observed if interpretation is not to dissolve into a chaos of assumptions and guesses. It is

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of fundamental importance to recognize that archaeological material can supply answers only to certain kinds of question and that it can throw light only on a limited range of human activities.

In the light of these limitations how are we to assess the claim of one archaeologist that, 'We are all historians now'? It is a claim echoed in varying forms by many archaeologists today, though a few critical protests are heard from purists and from the remote fastnesses of prehistory. We can understand what he means if he is trying to emphasize that his studies, unlike those of the geologist, are concerned primarily with human activities. We can understand what he means if he is trying to say that, like the historian, he is seeking to reveal sequences and relationships in human affairs. In these cases we can understand what he means and we can accept it, though we cannot accept the form in which he says it. If, however, he means that he proposes to obtain from archaeological evidence the same kind of conclusions as the historian obtains from historical evidence, then we must reject the thought behind the claim as firmly as we reject the form in which it is expressed. The fact of the matter is that an archaeologist is not and cannot be a historian. As the terms are understood today one deals with material evidence and the other deals with written evidence, and in this lies the essential difference between them.

A relentless determination to arrange human affairs into logical and mutually exclusive compartments often produces only an artificial appearance of consistency which does violence to what it seeks to clarify. The French definition of *préhistorique*, though universally applauded by scholars as an example of French logic, is an even better example of how confusion can be created

by an insistence on superficial forms and literal meanings at the expense of fundamental issues and true meanings. Let it be emphasized at once, therefore, that to say that an archaeologist cannot be a historian and that a historian cannot be an archaeologist is true only in the sense that they deal with different kinds of evidence. The same man can be both, of course, and it is highly desirable that this should be so. Indeed it must be so if one is to use the other's evidence for the elucidation and elaboration of his own. The present point is simply that it would be misleading to assume, because a man may function in two capacities, that the two capacities are the same and that he can switch from one kind of evidence to another without recognizing that it is different in character and that it is controlled by an entirely different set of limitations.

In the above survey of archaeological evidence the discussion has been directed towards problems which have an immediate bearing on the difficulties of co-ordinating archaeological and non-archaeological evidence. Therefore problems of interest more exclusively to archaeologists, such as those which centre on the search for an absolute chronology, have been left on one side. A few other problems, such as those which concern typological classifications and distribution maps are carried forward to the next chapter where they may be conveniently compared with the problems of the place-name scholar.

IV

LINGUISTIC EVIDENCE

IN the last chapter the discussion turned on the fundamental nature of archaeological evidence examined in the light of general problems of interpretation. It is not necessary to examine linguistic evidence in the same way, for we are concerned only with that part of it which is embedded in place-names. But, as our main concern with archaeological evidence was to consider how its nature controls its use in attempts to co-ordinate it with historical evidence, so our main concern now is to consider how and under what conditions the evidence of place-names may be co-ordinated with that of history and archaeology. We are not concerned with the contribution of place-names to lexicography, to an understanding of the principles and processes of word-formation and name-giving, to studies of dialects, dialectal variations, phonological developments and semantic evolutions, or to any of the problems arising from the form and content of language. Many philologists would see the true justification of place-name studies in the contribution they make to

other linguistic studies. They may well be right, but our concern is with place-names as they may be used to throw light on human endeavour in fields common to history and archaeology.

Of the recovery and recognition of place-name evidence little need be said. It consists of forms or early spellings of place-names, and these occur in a historical context, i.e. in documents both national and local and, occasionally, in inscriptions on stones and coins. Their collection involves a laborious search through printed documents and archives of unpublished documents; it presumes an ability to read difficult hands with accuracy; and it requires a knowledge of local topography and historical associations, for in some cases it is difficult to identify a particular spelling with its modern equivalent, especially if the former is distorted or the latter is a common place-name like Ashton, Newton, Norton, Sutton, etc. The collector of place-name forms, however, requires no highly specialized techniques like those of the excavator, and it is not necessary for him to interpret his forms during the process of their collection, though this of course adds greatly to the interest of collection and, if he knows where the problems of interpretation lie, it enables him to save time by ceasing to collect forms when a certain phase in the development of a name is sufficiently well attested.

There is a general impression in some quarters that a place-name scholar reaches his conclusions either by intuition or by some esoteric *mystique*. Nothing could be further from the truth. The impression, now surely current only among the exceedingly ignorant, probably arises from the fact that when a place-name scholar is asked to explain a name either he gives a short and

apparently dogmatic answer or he baffles the questioner with rounded and broken vowels, genitival inflexions, assimilations, *i*-mutations, increasing diphthongs and other forbidding technicalities. He is not guessing, and he is not simply working backwards from the modern form of the name. He is (or should be) speaking from his knowledge of the collected forms of the name. Occasionally, if the question concerns one of the more common names like the above-mentioned Ashton, Newton, Norton or Sutton, he may guess from his general knowledge of place-name patterns, but even a common name like Ashtoo may deceive one who merely guesses—the first element is usually OE *æsc*, ‘ash-tree’, but sometimes it is a personal name or another word. The fact of the matter is that behind the place-name scholar’s derivation there is a formidable mass of evidence in the shape of dated early spellings, and without such evidence no reputable scholar will seriously advance a suggestion. The collection of place-name forms of an area—for the field-names and other minor names as well as for the major names—is a task far more arduous than is generally realized. Only those who have collected the place-name forms for a county, with the detailed documentation that is required today, can have any idea of the work involved. That a great deal of the work of collection is monotonous must be admitted, but it provides occasional moments of joy when unusual or significant spellings appear to reveal a new facet of a problem or to bring support to a suspected derivation or development.

The techniques used by the place-name scholar to interpret his evidence in terms consistent with itself, that is in terms of language, need not delay us here. It was impossible to examine as close quarters the specialized tech-

niques of the archaeologist (see above, pp. 23-6) or the critical apparatus of the historian, and those which the place-name scholar applies to his material are very much more complicated. Detailed discussion of them here is therefore quite out of the question, but it is especially important that anyone who wishes to use place-name evidence for non-linguistic conclusions should make himself familiar with the principles on which these techniques of interpretation rest.

What the place-name scholar is seeking is not only the original form and meaning of a name; he is also trying to reveal the various phases of its development and the various linguistic influences that have transformed it through the ages. Indeed, in most cases he must understand the development before he can approach the original form with confidence. And he is, of course, no less interested in names which have fallen out of use and in forms which cannot now be identified with a current name or with any particular place or feature. To him all place-names, however ephemeral they are and however insignificant are the physical features to which they are attached, are valuable evidence which may throw light on the nature and development of language. He may arrange his material chronologically, territorially or according to types of one kind or another, he may classify it according to constituent elements or other features, and he may express his derivations in forms which would be appropriate only to an earlier age or another area; but these are conventions and, though they may mislead the non-specialist, they are no more confusing to the place-name scholar himself than current and strictly illogical phrases like 'Secondary Neolithic Peterborough ware' are to the archaeologist.

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The important point is that, since the place-name scholar's material is linguistic, his approach to it must be linguistic. To the interpretation of it he must bring a detailed knowledge of phonological changes and morphological variations, he must be prepared to make full use of the methods of comparative philology, and he must allow for the operation of special influences to which place-names are subjected (see above, pp. 10-11). His approach, one may emphasize by repetition, must be linguistic. But it must not be exclusively linguistic, for he must pay attention also to topographical and other non-linguistic evidence. It is a pointless academic exercise to urge that a certain interpretation is linguistically possible, even probable, if we happen to know from other sources that it is the wrong one. Some scholars of an earlier generation adopted such an exclusively linguistic attitude, and at the beginning of this century one went so far as to treat the place-names of Lancashire as 'a purely linguistic problem', declaring (somewhat ungrammatically) that his book 'is not concerned with the question whether the names fit the places to which they are attached, nor whether they ever did so'. This attitude produced such fantastic derivations as Altcar (on the River Alt) and Cockerham (on the River Cocker) from personal names 'Alta' and 'Kok' respectively. It probably arose as a not unreasonable protest against the wild guesses of what the author called 'amateur researches', but it carried at least one learned professor into cloud-cuckoo land with the 'antiquaries' whom he sought to restrain.

To compare the place-name scholar's problems with those of the historian and the archaeologist is useful because it throws light on all their problems and brings into prominence the difficulties of co-ordination. The first

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point, already noted repeatedly but so important that it cannot be too strongly emphasized, is that historical evidence is documentary and therefore direct evidence only of a state of mind, that archaeological evidence is material and therefore direct evidence only of practical skills, technological processes, aesthetic interests and physical sequences, and that place-name evidence is linguistic and therefore direct evidence only of language and speech habits (see above, pp. 6-11). Indirect inferences may be drawn in each case, and the evidence of place-names may be used to throw light on the date, nature and extent of settlements, on the movements of peoples and their relationships to each other, on certain aspects of their organization and on many of the other problems that concern the historian and the archaeologist. But in all these cases the inferences depend to some extent on assumptions and they must be examined carefully before they are accepted as valid.

Secondly, it has been pointed out above (pp. 9-10) that the linguistic element in place-names consists essentially of sounds and that, except in so far as modern pronunciations are taken into account, they are preserved only in historical contexts. The difficulties of interpretation are increased not only by recognizable linguistic influences (such as Norman-French influences which affect spellings in Domesday Book) but also by what must be described as mistakes or inaccuracies in the written forms which now provide the only direct evidence of what the sounds once were. In this sense, therefore, the place-name scholar's basic material comes to him at second hand, a situation which does not face the archaeologist who usually has direct access to his basic material. Like the historian, though to a lesser extent, he is concerned

with the trustworthiness of his evidence itself. It cannot lie, but it may be distorted. In seeking the original form and meaning of a name the earlier spellings are rightly regarded as more revealing and more reliable than later spellings, but the possibility of distortion arising from errors and inaccuracies carries a warning against placing too much reliance on the testimony of a single form. Sometimes a philologist will argue that this or that form cannot possibly belong to a certain name, when in fact it can be proved that it does; and in such cases a rigidly linguistic approach will lead only to confusion. The place-name scholar must allow for occasional distortions in the written forms which represent his basic material.

Thirdly, it has been noted that the techniques of collection employed by the place-name scholar are far less specialized than those of the archaologist. The value of a collection of place-name forms may be impaired by carelessness and incompetence, however, and in practice the scholar must either collect the forms himself or be sure of the efficiency and thoroughness of his assistants. Forms inaccurately copied or incorrectly dated can produce misleading evidence. It is important also to know the nature of the document from which the forms have been abstracted and whether or not all forms in it have been abstracted. Failure to pay sufficient attention to such points as these increases the difficulties of interpretation far more than do the occasional distortions in the written sources themselves. The tremendous labour involved in the compilation of a place-name collection today makes it necessary to use assistants at some stage, but the possibility that relevant incidental information may be missed is always present and the responsibility of deciding how and when to call in assistance is one that the place-name

specialist must accept. The archaeologist has a similar, though by no means identical, problem in that he is almost always compelled to make use of assistants in his work as an excavator.

A fourth point arises from the nature of the evidence. It was implied above (p. 7) that those archaeologists who describe their material as 'unconscious' are guilty of mis-using the word. There is no need to beat about the bush: the word applies to a mental state and cannot be applied to an inanimate piece of pottery. We know what they mean of course. They mean that the pot conveys to the archaeologist information not intended by the men who made it and that in this respect it is different from historical evidence. They overlook the fact, however, that all written records convey to the historian information not intended by the men who composed them and that this is as true of chronicles and avowedly expository works as it is of documents which attempt only to record. They also ignore the difference between doing something with an eye on the future and doing it deliberately to provide evidence for a future scholar, and they apparently forget that much archaeological material was made with an eye on the future. Leaving aside such considerations, however, let us for a moment examine how far the word 'unconscious' might be applied in a metaphorical or transferred sense to historical, archaeological and linguistic material respectively. The man who composes a chronicle or a charter is conscious of what he is doing. So equally is the man who makes a pot or carves a stone. Either or both may consciously have an eye on the future, and neither is likely to be consciously providing evidence for a future scholar. Now this is where place-names seem to stand apart from both historical and archaeological material: it

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goes without saying that place-names were not created as evidence for a future scholar, but neither were they created for the edification or admiration of a future generation and, most important of all, the people who created them were not even conscious that they were creating place-names. The point is that place-names, unlike pots and manuscripts, arise spontaneously without conscious intention on the part of those who make them and they develop without a conscious attempt to mould them to a particular plan or design. There are occasional examples of names deliberately formed or changed, but these are comparatively rare exceptions to the rule. This spontaneity and lack of calculated human interference seems to be one of the most important characteristics of place-names. Its significance in terms of interpretation will become apparent below.

Another characteristic, more difficult to put into words, is connected with the last. Place-names are direct evidence only of linguistic developments, it is true, but within this comparatively narrow field they have a generality, not to say universality, that is denied to archaeological and historical evidence. One is aware that most people use pots and that many may read or be mentioned in manuscripts, but both pots and manuscripts are made by specialists and it is the work of these specialists that comes before the archaeologist and the historian. Place-names by contrast are not produced by specialists but by ordinary men and women speaking their own language as they spoke it naturally every day and bringing no special skills and techniques to the formation of place-names. What is especially difficult to put into words is the point that the spontaneity and lack of calculated human interference in place-names, coupled with this fact that they are more

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closely representative of the general linguistic aspects of human activity than archaeological evidence is representative of its general material aspects, allows one to use them as indirect evidence with greater confidence. That is to say, conclusions about political and social developments reached by indirect inference from place-names are on the whole more reliable than similar conclusions reached by indirect inference from archaeological evidence. Or, to put it in another form, assumptions are inevitable in all conclusions of this kind, but as a rule the assumptions behind non-linguistic inferences from place-names are less prominent and less strained than the assumptions behind non-archaeological inferences from archaeological material.

A sixth point arises from the qualities of spontaneity and generality in place-names, and it provides another argument for the reliability of non-linguistic conclusions based on them. One may, if one wishes, visualize the historian, the archaeologist and the place-name scholar as all engaged in a similar kind of activity, that of trying to recover sequences, relationships and explanations. Just as the excavating archaeologist is engaged in stripping off and examining layers of archaeological evidence from his site, so the historian is engaged in stripping off layers of accretions and modifications from his text, the place-name scholar is engaged in stripping off layers of phonological development from his names, and all are trying to uncover original forms and the various influences to which they have been subjected. In this activity the place-name scholar has one very great advantage over the historian and the archaeologist. In the primary interpretation of his evidence he can call upon—indeed he must call upon—a body of well-defined rules that govern almost

all aspects of linguistic developments. The historian and the archaeologist in their interpretation of manuscripts and sites respectively have no such assistance. They may call upon their knowledge of other sites and other manuscripts, and their techniques may be applicable to other sites and other manuscripts, but every site and every manuscript is unique in the sense that it is a law to itself. A place-name scholar can say in effect, "This or that linguistic development is known to occur generally with the force of a law and therefore in this instance we may safely conclude that . . .". An archaeologist who tried to explain sequences on his site by such an appeal to general rules would soon be in dire trouble. And so would a historian who employed the same kind of argument to elucidate the manuscript relations of his chronicle.

The philologist is often impatient with the historian and the archaeologist, blaming them for slovenliness of approach and lack of precision in their conclusions. Perhaps he would be more tolerant if he remembered that his own precision comes largely from the existence of a body of rules of general applicability and a body of comparative material which can be brought to bear directly on his own particular problems, an advantage which excites the envy rather than the admiration of the historian and the archaeologist. It will be seen that this advantage springs from what have been called above the spontaneity and the generality of linguistic material. It will also be seen that the increased precision which results from this advantage gives a greater validity to inferences drawn from place-name evidence.

One of the archaeologist's approaches does, indeed, attempt to supply something of the deficiency that results from the absence of any well-defined and widely applic-

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able body of rules governing the development of archaeological material. This is the typological method, the basis of which is an attempt to arrange archaeological objects such as weapons, implements and pottery in some kind of chronological sequence. There can be no question that the typological method has made great and permanent contributions to the study of archaeology, and in the hands of a master it can throw brilliant light on aspects of human activity that might otherwise remain in darkness. At its worst, however, it is no more than a ridiculous conceit. Its successful use demands an objective approach, and it is unfortunate that the most subjective of students often figure prominently among those most attracted to it. Even at its best it depends upon a series of assumptions, and these are not always strong enough to support the strain imposed upon them. There is what the archaeologist calls an 'irreversible typological series', proposed on the assumption that certain observed developments can take place in one direction only, that is forwards but not backwards, and other series are proposed on the unadorned assumption that changes exhibit a tendency towards increased efficiency or a tendency to advance from the simple to the complex.

When a typological series can be proved to be irreversible it carries great conviction, but irreversible series are claimed more often than proved. As for assumed trends, well, enough examples of diminishing efficiency, artistic degeneration, rejection of the complicated and revival of the simple can be produced to undermine any belief that trends in craftsmanship possess anything like the compulsion inherent in either the evolutionary systems of the natural scientist or the phonological developments of the philologist. One must also bear in mind the impact upon

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each other of peoples at different technological levels, the effect of isolation and the emergence of local variations, to say nothing of the man who consciously rebels against current fashions and the man who follows his own imagination rather than the models before him. Typological sequences in archaeology are most reliable when the bases of the assumptions behind them are functional, for in this field there are strong forces operative, if not irresistible compulsions, and in this field also the typologist has at least a chance, sometimes a good chance, of making fairly objective assessments. They reach the nadir of reliability when attempts to arrange artistic designs in chronological sequences rest only on the assumption that the artists were bound by evolutionary criteria that have no existence outside the typologist's own mind. It must be admitted that it is usually art-historians who produce the most subjective and the most unconvincing sequences. Not all art-historians of course. And not only art-historians. They have no monopoly of self-delusion.

It should be made clear that the archaeologist's typological systems have little in common with the evolutionary sequences of the natural scientist, from which they were borrowed, or the phonological developments of the philologist, to which they have been compared. The latter have nothing to do with function, as the archaeologist understands the word, and they have less to do with illusions that straight lines tend to become curved or that curved lines tend to become straight. In common with the evolutionary sequences of the natural scientist they are divorced from conscious human influence and interference; it is this that gives them the element of compulsion from which is derived their wide

applicability and, therefore, their comparatively high degree of reliability. The archaeologist's material is too closely dependent on conscious human endeavour to permit its escape from the vagaries implicit in that relationship. The historian's material is even more dependent on volition, and the historian has always realized that what he calls the human factor precludes the postulation of precise and compelling patterns in the affairs of men. The student of charters, for example, is wary of pressing a conclusion based only on diplomatic sequences if other evidence fails to corroborate it. The archaeologist's material, it has now often been emphasized, directly represents practical skills, technological processes, aesthetic interests and physical sequences, but changes in these fields are not altogether divorced from the conscious human element of volition, comparable to the historian's 'state of mind', and in this lies the crucial difference between them and the phonological developments of the philologist.

The point is that the archaeologist's typological systems, even at their best, cannot have the force of a natural law and cannot be expected to reveal the precise chronological significance of every relevant object submitted to them. This is not to say that typological studies have no value. Such a position would be untenable in the face of the results made possible by typological classifications. They are always useful for purposes of description, illustration, comparison and corroboration, and very frequently firm chronological conclusions can be drawn from the application of comparative material to individual objects. But this is another matter. The validity of a conclusion thus obtained depends upon the validity of the comparison and of the assumptions based on it rather

than upon any inherent compulsion or inevitability in the series itself.

The place-name scholar makes use of similar classifications, but he does not confuse them with phonological developments. He seeks to recover and place in some kind of chronological order the predominating patterns of nomenclature, recognizing that these change from area to area as well as from age to age, and he advances by considering the nature, usage and significance of names which contain, for example, such elements as *-ingar*, *-ingabām*, *bām*, *tūn*, *būr* and *þorp* (on which see below, Chapters V and VI). But the criteria behind his chronological classifications are not primarily phonological; they arise from assumptions based on political and social conditions, the incidence and character of personal names, geographical distributions, topographical associations and comparative material of various kinds. The typological classifications of the archaeologist have much in common with these place-name classifications. They serve a useful descriptive purpose, and they provide bases for permissible interpretations and conclusions, but in all cases they depend upon assumptions which, varying from instance to instance, must each be subjected to a severely critical examination. They do not have the force of a 'law' as the word is understood by scientists and philologists.

Distribution maps are sometimes criticized under the same headings as typological systems, but this is inapposite, for by themselves distribution maps add nothing to the discussion. Their purpose is to bring graphically to the eye relationships that exist or may exist between certain selected features and other features. Their value lies in the fact that graphic representation of relationships facilitates their comprehension by the mind, and in some

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cases the relationships are so complex that they cannot be comprehended except by graphic representation. Distribution maps do no more than express the location or space relationships of given features, and they might equally well be called location maps, as they often are. There is nothing wrong with the name distribution map, however, so long as it is not invested with some mystical or magical property of revelation which it does not of itself possess.

It is not distribution maps that need to be criticized but the inferences that are sometimes drawn from distributions or, rather, the assumptions on which these inferences rest. In seeking conclusions one must be sure that the selected features belong to the same period or, if the intention is to trace changes in time, that the selected features belong respectively to the periods concerned. One must be sure, too, that the features can support the inferences drawn from them and also that other factors do not exist to destroy their validity. Conclusions based on too few examples may be misleading; they may reflect no more than the accidents of survival or discovery, and sometimes one suspects they really reflect the fact that antiquaries have been more active or more enthusiastic in one area than in another. Frequently, especially in Scotland, they represent only geographical or geological limitations or the occurrence of fertile soils, and in these cases it is the differences between archaeological distributions rather than their similarities that are most significant. It is necessary, for example, to be sure that coastal distributions do represent sea-borne influences, not merely geographical limitations imposed by a narrow coastal plain. In seeking political conclusions it is especially important to be sure that the selected features can support

the inferences drawn from them. Changing forms and decorations in pottery, for example, may arise from the influences of trade or expanding contacts rather than from political unities, and even with structures like brochs and hill-forts the assumption that they reflect political organizations requires support from other sources. Much depends upon the nature of the features selected, the number of examples available and the amount of other evidence that can be brought to the support of the inferences drawn. Conclusions based on place-name features are open to many of the objections that might be raised against conclusions based on archaeological features, but reasons have been given above (pp. 45-48) for accepting linguistic features as more closely related to the political and social aspects of human activity. To that extent, therefore, and speaking broadly, conclusions based on place-name distributions are more reliable than conclusions based on archaeological distributions.

A final point in this general survey may be made concerning the completeness of the evidence available. Neither the archaeologist nor the place-name scholar can draw conclusions from evidence that no longer exists or is not available, though both must try to allow for it in their conclusions. Both have lost much evidence by the passage of time, and if the archaeologist is fortunate in that the survival of his evidence is entirely independent of the survival of a written record the place-name scholar is better equipped to grapple with the unknown by inference. And this also applies to material which, though it has survived, presents problems of identification. The term 'ritual object' has been laughed out of court to such an extent that its use by an archaeologist today is taken as a confession that he has no idea at all what the object is.

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There was a time when the 'hitherto unrecorded personal name', postulated by place-name scholars to explain an obscure first element, was coming to be regarded at least by non-specialist critics as a similar confession of ignorance. In the last twenty years Scandinavian scholarship, which has made so great a contribution to the study of English place-names, has taught us much about genitival composition and we now know that non-personal first elements in the genitive case are much more common than used to be thought. But at no time has any reputable place-name scholar doubted the existence in place-names of a considerable number of personal names which happen not to have been recorded independently. He can bring convincing support to his derivations from the known principles of name-giving and from comparable forms and name-stems preserved elsewhere. The archaeologist is in a different position with regard to ritual objects: by their nature they can be explained only in terms of ritual, and if this is unknown there is no more to be said. It should not be forgotten, however, that archaeological objects of unknown purpose can still be used to provide information about technical capacities and aesthetic interests, and that place-name elements of obscure significance can still be used to provide linguistic information. But it would be unwise to attempt to draw more general conclusions from them so long as they remain obscure and unexplained.

V

BRITONS, ANGLO-SAXONS AND PICTS

IN the last chapter attention was drawn to certain characteristics of place-names. These are important to all who attempt to interpret them—in terms of language or in terms of non-linguistic conclusions. It was emphasized that place-names are essentially linguistic material and therefore direct evidence only of language and linguistic developments. But when place-names are used for non-linguistic conclusions, that is as indirect evidence, it was suggested that they often inspire greater confidence than one would accord to non-archaeological conclusions from archaeological material. On balance, in other words, the historian may prefer to put his trust in conclusions drawn from place-name evidence. This does not mean that the historian or anyone else is entitled to collect examples of British, Anglo-Saxon, Pictish, Scandinavian and other groups of place-names, put them indiscriminately on maps, and then hopefully expect that simple answers to complicated questions will leap automatically

to his eye. Other considerations must be taken into account if reliable conclusions are to be drawn from place-names, and it cannot be too strongly stressed that their interpretation in terms of non-linguistic conclusions offers many hazards to the unwary.

Some of these considerations are so elementary as to be almost self-evident. It is true that claims to have discovered Phoenician, Mongolian and other surprising influences in the place-names of Britain are now put forward only by maniacs, but strange conclusions still come out of the bags of those who forget the basic principles and conditions behind name-giving. The fundamental purpose of names is to provide a means of identification; this is the function that dictates their formation and keeps them alive. We may not know the immediate requirements of identification behind an individual place-name, but if our interpretation of it or an assumption based on it conflicts with these underlying requirements we are likely to go astray. Secondly, it should be remembered that place-names, arising spontaneously as identificatory labels, are part of the language of the people(s) who formed and used them. They may incorporate elements from another language or speech group, and this is always significant, but it does not of itself carry them into the language of their constituent elements. An English-speaking community, for example, may incorporate British elements or Scandinavian personal names into some of its place-names, but the place-names as such are English. And they remain English wherever and for so long as they are in common use by an English-speaking community.

Thirdly, the existence of a name depends upon its use and acceptance by people outside the place to which it is

attached, i.e. by the people who use the name as an identificatory label rather than by the people who happen to live in the place. The name of a village, for example, depends for its existence upon the people in the neighbouring villages, not upon those who live in it; the latter are usually content in conversation to refer to it as 'here' or 'home' or 'this village' as distinct from other places. It is the size of the area or population throughout which a place-name is accepted as an identificatory label that determines its persistence and permanence. That is why river-names, hill-names and names of other prominent topographical features, having a wider currency, are so persistent and so permanent. At the other extreme are field-names and other minor names which, being current only among a comparatively small number of people, lack the wide acceptance that makes for permanence. That is why they are so frequently forgotten, lost, changed or replaced. Between the two extremes are the great majority of village-names, much more permanent than minor names but seldom so long-lived and persistent as the names of hills and rivers. These divisions have nothing to do with formal and functional differences, only with the currency value of the various names. Small streams often have names as impermanent as the names of fields; towns and large villages are sometimes as widely known as rivers and hills; some village-names and hamlet-names have no more than a local currency and they tend to disappear with the settlements to which they are attached; and on the other hand many names which formally are minor names (e.g. Bradford, Oxford, Henley, etc.) have gained a wider currency than most major names.

In attempts to base non-linguistic conclusions on place-names these and similar considerations must be

borne in mind, for the reliability of the conclusions proposed depends upon a just appreciation of the conditions underlying the forms and developments of the names as well as an understanding of the forms and developments as linguistic phenomena. Linguistic superiority in an area, for example, need not necessarily imply political dominance—consider England after the Norman Conquest—and even the complete disappearance of a language need not imply extermination—consider the linguistic fate of the Picts. On the other hand, the survival of fragments of an earlier language in place-names, though it must imply intercourse or contact of some kind, need not imply the survival of the people who spoke the earlier language; in many cases, no doubt, they were absorbed into the new society, giving up their language and retaining their lives, but often there is nothing in the linguistic evidence alone to preclude an assumption that they were exterminated at some date after contact had been established. It will be clear that the non-linguistic interpretation of place-names raises very many problems—too many to enumerate here—and that each must be considered on its own merits and within the framework of evidence from other sources. The remaining pages of this chapter and the whole of the next are devoted to illustrating some of those problems by means of examples. It is emphasized that the examples are not intended to cover all the problems, and they are certainly not intended to give an impression of the extent and range of the potential contribution of place-name studies to history. They are examples only, and the object is simply to explain and illustrate the kinds of problem that one frequently meets in attempts to use place-names for conclusions of a non-linguistic character.

One of the most crucial problems of early English history is that of the extent to which the British population survived the impact of the Anglo-Saxon settlement. Contributions to its solution come from several different sources, including that of place-names which alone concerns us here; and it is at once clear that place-names by themselves cannot provide a complete picture and cannot answer all the questions sometimes addressed to them. The most persistent Celtic names are those of prominent natural features such as rivers, hills and forests, and with them we may include names of ancient territorial divisions and names of Romano-British towns. Such names are found in all parts of England, but they are comparatively rare in eastern and south-eastern areas, which bore the initial brunt of the Anglo-Saxon conquest, and they increase in number only as one approaches the western shires. They point to intercourse or contact of some kind between the native Britons and the advancing English, and one may fairly assume closer or more prolonged contact in the western shires which the English settlers reached in smaller numbers or at a time when the force of the conquest had lost much of its violence. But they do not in themselves imply very close contact, even in the areas where they are most common, and they do not provide direct evidence for the survival of a British population. On the other hand, even the complete absence of Brittonic elements in some eastern or south-eastern areas does not necessarily imply that the Britons were exterminated.

When one turns to habitation-names, names of villages and homesteads, these are rarely found to contain Brittonic elements, even in Devon, Somerset, Gloucestershire, Lancashire and the shires adjacent to Wales, and it is a notable fact that they present certain rather surprising

features. In the whole of Lancashire, for example, less than sixty place-names containing Brittonic elements have been noted, and by origin as well as by usage most of them were strictly English place-names, i.e. names like Cheetham, Pendleton and Chadderton (containing **ceto-*, **penno-* and **cader* or **cadeir* respectively with an English final element) as distinct from names like Culcheth, Penketh and Tulketh (in which both elements are Celtic). An even more remarkable feature of Brittonic elements in Lancashire place-names is that almost all of them refer to natural or prominent topographical features, as for example **ceto-* ('wood'), **penno-* ('hill'), **egŷes* ('church'), **inis* ('island'), etc. Hardly any of them refer to any kind of habitation (Treales, cf. Welsh *tref*, 'hamlet', and *llys*, 'hall', is perhaps the only safe example), and hardly any of them are personal names (Wigan, apparently an elliptical form of an earlier *Tref Wigan* or *Bod Wigan*, may be one example). This is a most important feature when one comes to interpret these names in terms of history. The failure of genuine Brittonic formations to impress themselves on English place-nomenclature and the monotonous repetition of topographical elements in the so-called British place-names carry a strong warning against overestimating the extent to which the Britons and the English were in contact with each other when such names arose. If any number of Britons had survived in close contact with the English one would have expected the preservation of more than an occasional Brittonic habitative element and more than an occasional Brittonic personal name. The rarity of Brittonic habitative elements is especially relevant to the question of whether or not British villages and settled communities survived the impact of the Anglo-Saxon conquest.

The warning against overestimating the contact between the two peoples is underlined, in Lancashire and elsewhere, by such common tautological formations as Cheetwood and Chetwoode ('wood-wood'), Bredon and Breedon ('hill-hill') and Pendle and Penhill ('hill-hill'),¹ for they imply that the neighbouring Anglo-Saxons had heard but did not know the meaning of these common Celtic words. They count heavily against all but the most brief and superficial contact. In marked contrast, as evidence of intercourse between the two peoples, is a name like Dover (OE *Dofras*, Romano-British *Dubris*, British **Dubrās*, Late British **Dobrās*) which implies that the Anglo-Saxons who adopted it could recognize Celtic plurals in oblique cases. The name Dover might be discounted as familiar to Anglo-Saxon raiders and traders long before the *adventus Saxonum*, but this argument cannot apply to inland names like Andover and Wendover, for which a similar adoption of Celtic oblique plurals by Anglo-Saxons must be postulated. The rarity of such formations, however, throws into high relief the isolated character of the great majority of Brittonic elements in English place-names.

We know, of course, from other sources of evidence that a considerable number of Britons survived in some parts of England. We know, too, that a people can physically survive the impact of a conquest and yet leave few traces in the place-nomenclature of an area, for these depend less upon physical survival than upon the relations between the conquerors and the conquered, the nature of the contact between them, the tolerance of the

¹ In a later age the tautological Pendle (British *penw*, OE *hyl*) received a secondary explanatory 'hill', with the result that Pendle Hill means 'the hill hill hill'!

one and the readiness of the other to accept assimilation, the question of which language is given up and, above all, the question of how quickly and how thoroughly the process of linguistic assimilation is carried through. It is important not to base positive conclusions on place-names when the only evidence they offer is negative. The point is that those who seek to prove a very considerable British survival in England must find support for their thesis in evidence other than that provided by place-names.

From place-names we can legitimately infer a certain amount of contact between the two peoples, varying in degree from area to area and increasing as the English advanced towards the western shires. There we may assume that in the later phases of the settlement there was less violent hostility and that the English arrived in comparatively smaller numbers or held a relatively less dominating position numerically, with the result that the linguistic balances were not quite so heavily weighted against the survival of Brittonic place-name elements. We can also draw fairly safe conclusions about the nature of the contact between the two peoples. In Lancashire, for example, the distribution of the Brittonic elements suggests that the Britons were not driven into the hills and other remote areas, as is so often urged, and that their proximity to what we may take to be the earliest English place-names in the area strongly suggests that linguistic contact was more or less confined to the first phase of the settlement there. Whether the Britons were then expelled and/or exterminated or whether, as is on general grounds much more likely, they quickly and completely adopted the language of their conquerors is not for the place-name scholar to say on the purely linguistic evidence be-

fore him. What he can say, and that with some emphasis, is that however many Britons physically survived the Anglo-Saxon conquest nowhere in England is there any sign that they exercised an appreciable influence on the language, economy, political arrangements and general way of life of the English. The Romano-British communities were completely broken, and with their language they lost their national identity and their corporate existence. Where they survived it was as alien elements in a society essentially English and essentially Germanic.

Into the above discussion come other factors not considered here, and in particular the question of the dates at which the various Brittonic elements were adopted into English place-nomenclature. It will be obvious that Welsh place-names arising from advances eastwards across Offa's Dyke at a later date must be excluded from the discussion, but it is more difficult to assess the linguistic developments which affected Brittonic elements in the age of settlement. The history of Brittonic and Anglo-Saxon sound-changes in these dark centuries is not yet fully understood, and we must leave these problems to specialists in Celtic and Anglo-Saxon philology. The importance of the chronological factor in non-linguistic interpretation may be more clearly illustrated by reference to attempts to recover from place-names information about the rate and progress of the English settlement. The underlying theory is simple enough, i.e. that the distribution of the earliest English place-name formations will indicate the areas of earliest settlement, that chronologically secondary formations will indicate secondary settlement, and so on. Therefore the place-name scholar is concerned to disentangle the changing patterns of place-nomenclature and to arrange them in some kind of

chronological order, a process which requires a consideration of how the habits of name-giving react to changing social and economic conditions as well as to linguistic developments.

This is more complicated than might appear and, as the immediate object is simply to provide an example of the importance of the chronological factor, we may leave on one side place-names which preserve references to Anglo-Saxon heathenism and a number of elements which seem to belong individually to the migration age, and concentrate attention on the considerable number of names which originally ended in the plural termination *-ingas*. For more than a hundred years historians have been aware of these names, but their interpretation has been much modified since Kemble drew attention to what he called 'the patronymical termination *ing*'. It is not enough to count up place-names which today have an 'ing' in their forms, for many of these are etymologically incorrect; they may represent, for example, a weak genitive singular (*-an*) as in Abingdoo (OE *Abbandūn*, 'Abba's hill'), a weak genitive plural (*-ena*) as in Huntingford (OE *Hunttenaforð*, 'the ford of the hunters'), an adjective like **cærsen* ('cress-growing') in Carsington or **gæræn* ('grass-growing') in Garsington or **hæslen* ('hazel-growing') in Haslingden, or some other word like OE *acorn* ('acorn') in Accrington or ON *bringr* ('ring, circle') in Eakring or ON *eng* ('meadow') in Mickering. There is also the noun-forming suffix *-ing*, in common nouns like *teþing* ('market'), *bygging* ('building') and *stæbbing* ('clearing'), which occurs frequently in place-names (e.g. Chipping, Newbiggin and Stubbing), especially in minor names, and is nearly always indicative of late formations. There is, however, a singular place-name-forming suffix *-ing*, possibly an adaptation of

the noun-forming suffix *-ing*, perhaps identical in function with it and often difficult to distinguish from it. Examples are Clavering, Kemsing, Bletching and Nursling. Some of the examples are palatalized, like Geddinge, Lockinge, Ruckinge and Wantage (*Waneting* < *Waneting*), and this may be indicative of an old locative case (e.g. *at Waneting*) or perhaps of a distinction, observed in Swedish parallels, between nature-names (which are *a*-stem declensions) and old settlement-names (which are *ja*-stem declensions). Some of these names are comparatively late formations, but many are undoubtedly early. They occur in most parts of England, but their greater frequency in Kent, Hampshire and Berkshire has led to the suggestion that they bring support to Bede's statement that the men of southern Hampshire and the Isle of Wight were of the same Jutish stock as the men of Kent. It seems clear that they have a bearing on the course of the Anglo-Saxon settlement, but it is not always possible to decide which examples are early and which might be late. In any case they are formally quite distinct from the *-ingas* names in which the historian tends to put his trust, though he often confuses them with other formations.

The nominative plural *-ingas* is found in Reading (*Rēadingas*, 'the followers or men of Rēada'), Woking (*Woccingas*, 'the men of Wocce'), Hastings (*Hāstingas*, 'the men of Hāst (a)') and many similar names. Strictly they are folk-names, not originally place-names, for they referred to a people and only later to a specific place. For long it has been agreed that these names, arising at a time when the Anglo-Saxons saw themselves as organized in comparatively small communities held together by a personal bond, reflect the unstable conditions of the period immediately following the migration and therefore belong

to the first phase of the settlement. Hardly later in time, but continuing to arise for a rather longer period, are the specific place-names like Wokingham (*Woccingabām*), Goodmanham (*Gōdmundīngabām*) and Walsingham (*Waltingabām*), formed by the addition of *bām* ('village') to a folk-name in the genitive plural (*-ingas*). The chronological range of names in *-ingas* and *-ingabām* is dependent on conditions obtaining in areas where they occur, and it is probable that migration-age conditions would survive longer outside the areas of initial settlement and where the Anglo-Saxons were pushing westwards to occupy new lands. But it is generally accepted that names in *-ingas* are characteristic of the fifth and sixth centuries and that names in *-ingabām* are not likely to have arisen much after A.D. 625.

Other *-inga-* (genitive plural) formations no doubt belong to the same early period. Examples are Hastingleigh (*Hāstingaleah*, 'the clearing of the Hāstingas'), Hastingford (*Hāstingaford*, 'the ford of the Hāstingas'), Pangbourne (*Pāgingaburna*, 'the stream of the Pāgingas'), Redlingfield (*Rādlingafeld*, 'the open land of the Rādlingas') and Hoddingtoo (*Hoddingatūn*, 'the enclosure of the Hoddingas'). Some of these elements suggest communities long established on the land and already developing its resources. Most of them were alive and productive in the age of settlement, but many of them remained alive and productive as place-name elements throughout the Old English period. Therefore, although *-inga-* formations imply the existence of communities or groups of people, it cannot be assumed that all arose in the first phase of the settlement. The element *bām* early became obsolete as a productive place-name element, but the same cannot be said of elements like *leah*, *tūn*, *fild*, etc.

The problem is greatly complicated by the existence of a medial *-ing-*, a connective particle which has a quasi-genitival force, though it does not always indicate possession. So far as one can see, most modern 'ingtons' have to be derived from *-ingtūn*, not from *-ingatūn*. Tiddington in Warwickshire and Teddington in Worcestershire, for example, must be derived from *-ingtūn* (the *tūn* of or associated with Tida and Teotta respectively), not from *-ingatūn* (the *tūn* of the Tidingas and the Teottingas respectively). Such names are very numerous, and it is important to find criteria for distinguishing them from *-ingatūn* formations. It is not always possible to separate the two, for in some names Old English as well as Middle English spellings fluctuate between *-inga-* (*-inge- > -ing-*) and *-ing-* (*-ing-*, *-yng-*), and one cannot ignore the possibility in some cases of a reduction from *-inga-* to *-ing-*. Some of the *-ingtūn* names are early, a view which is supported by their frequency in south-east England, but their distribution suggests that many of them are late, and it is known for a fact that they were being formed as late as the eighth and ninth centuries. Names in *-ingtūn* (and other less common combinations with the medial connective *-ing-*) must therefore be used with caution. They are not indicative of group-settlement like names in *-ingas* and *-inga-*, and it must not be assumed that they arose in the first phase of the settlement.

All this is complicated enough, but it is by no means the whole story. Not all names in *-ingas* are derived from personal names, and examples like *Eoforwicingas* (applied to 'the men of York' in 918) and *Fisburgingas* (applied to 'the men of the Five Boroughs' in 1013) show that the use of *-ingas* to form colloquial expressions (meaning 'the men of', 'the people who live at', 'the townsfolk of', etc.)

remained alive and vigorous throughout the Old English period. If Hertingfordbury (*Hertfordingaburb*, 'the burh of the men of Hertford') refers to 'the northern burh at Hertford between the Maran, the Beane and the Lea', built by Edward the Elder in 911 (Anglo-Saxon Chronicle, MS. A, s.a. 913), then it is a clear case of a new *-ingas* place-name arising in the tenth century. It would also suggest that the colloquial use of group-names like *Eoferwicingas*, *Fisburgingas* and *Hertfordingas* was very much more widespread in the last century of the Old English state than has been supposed. It has recently been urged from another direction that we should reconsider our attitude towards *-ingas* names, for those which are derived from topographical features, including older local names, 'continued in use throughout the OE period'. Some of these non-personal *-ingas* names belong to the age of settlement, but many do not.

This is of some concern to the historian who, having heard about the singular suffix *-ing* and having decided not to meddle with the treacherous names in *-ing(a)ton*, has hitherto felt that he was safe in accepting as belonging to the age of settlement all genuine *-ingas* names approved by place-name scholars. It is now necessary to examine all *-ingas* folk-names before accepting them as belonging to the first phase of the Anglo-Saxon settlement. It may be that our opinions on the rate and progress of the settlement in certain isolated or outlying areas may have to be modified, but it is unlikely that the general picture will be seriously disturbed. How the picture drawn from place-names squares with other evidence does not concern us at the moment (see below, pp. 116-17). Here the point is simply that if we are trying to base conclusions of a political or social character on place-names it is important

to know whether or not they arose in the period with which we are concerned. If they did not, the assumptions involved will be of a different order.

We may turn from the Anglo-Saxons and the importance of evolving a chronology of place-name types to the Picts and the importance of realizing that linguistic boundaries may but do not necessarily coincide with known political boundaries. The element *pitt* (modern *pit*) in place-names like Pitewan, Pitkenney, Pitsligo, etc. apparently means 'a piece (of land)', as in Low Latin *pelia terrae* (cf. Welsh *peth*, Gaulish **petia*, Gaelic *cuid*), and it seems to have been part of a Gallo-Brittonic language distinct from that spoken by the Britons further south. Its distribution marks with some precision the boundaries of a linguistic province which stretches northwards from the Antonine Wall to the area around the Dornoch Firth in south-east Sutherland. Its southern boundary, the Antonine Wall, coincides most convincingly with the southern political boundary of the historical Picts, as this may be deduced from other sources of evidence (see below, pp. 117-18), but it does not tie up with any known political boundary in the north. The historical kingdom of the Picts extended northwards to include the rest of Sutherland, Caithness and the Northern Isles, but no *pitt*-names are found in these northern regions. The boundary between the 'northern' and the 'southern' Picts, on the other hand, was probably the impressive mountain barrier which runs out to the sea at Stonehaven, and it is significant that the distribution of *pitt*-names extends beyond and quite ignores this boundary. The explanation (on which see more below, p. 118) seems to be that there were two linguistic provinces in Pictland, one marked by the distribution of *pitt*-names in which a

Gallo-Brittonic language was dominant and another embracing the further north in which pre-Celtic non-Indo-European peoples were linguistically dominant. The point here is that a linguistic boundary may coincide with a political boundary, as it does in southern Pictland, but may cut across known political boundaries as it does in northern Pictland. The example carries a warning against assuming, without the support of other evidence, that a linguistic boundary necessarily reflects a political boundary.

It should not be assumed from the above that there was a clear-cut linguistic boundary dividing Pictland into a southern Celtic-speaking province and a northern non-Celtic-speaking province. The southern linguistic province is predominantly Celtic, but there are enough non-Celtic names there to show that the non-Celtic-speakers retained for a time their non-Celtic speech and must have formed a substantial element in the population. In the north, on the other hand, where the non-Celtic-speaking element seems to have formed the bulk of the population, there was at least a thin Celtic superstratum. The evidence quoted for the existence of Celtic-speakers in the far north usually includes tribal names like *Cornavii*, *Lugi*, *Smeriae*, *Decantae* and *Carnonacae*, all probably Celtic, all listed by Ptolemy early in the second century, and all to be located north of Inverness. Ptolemy also gives the name of the northern promontory as *Orcas* and the name of the Orkneys as *Orcades*. This, too, is Celtic, and it must go back at least to the first century before Christ, perhaps even to the fourth. There is good reason to believe that Celtic-speaking peoples had reached the far north long before the beginning of the historical Pictish period (c. A.D. 300) and, though they seem to have been submerged

linguistically by the earlier non-Celtic inhabitants, it is possible that the names quoted above were introduced by them. But that the men of Orkney and the northern peoples listed by Ptolemy were themselves Celtic-speakers at the time when the above names appear is a conclusion for which there is not sufficient warrant. In the case of Orkney, indeed, it is a conclusion that positively conflicts with all probability. The names quoted above show only that certain peoples were given Celtic names, presumably by their neighbours or strangers. This is evidence of Celtic influences of some kind in the far north, but it is not sufficient to bear alone the conclusion that the peoples so named were themselves Celtic-speaking. It may have been so, but the assumption involved is so strained that it renders the conclusion invalid. It would be as reasonable to conclude that the Britons of Wales spoke English simply because the name by which they were known to their English-speaking neighbours is the English word 'Welsh' (OE *Wēalas*, 'foreigners'). There is no need to labour the point.

A similar example of dubious assumptions may be taken from recent comments on the name Dumyat, 'the fortress of the Macatae (Miathi)'. Dumyat near Stirling, a prominent outpost of the Ochils, breaks the skyline at over 1,300 feet and carries on its summit the remains of a fortress of unmistakably Dark-Age character. About ten miles to the south is the conspicuous isolated height of Myot Hill, also with its fortress, although this has not yet been firmly attributed to a specific period. The etymology of the name Dumyat is not in dispute, but about eight years ago two students of the Picts attempted to assess its historical implications. They remain good friends, which is not always the case with students of the Picts, and so it

is permissible to refer to the two quite different interpretations. It is the assumptions behind the conclusions that are significant in this context. One scholar argued that, as there must have been many fortresses within the territory of the *Maetae*, there must have been some good reason why they described one specific fortress as 'the fortress of the *Maetae*'; he assumed, first of all, that the name arose among the *Maetae* themselves, and then he assumed that the name was a measure of its importance. He concluded that Dumyat was 'an important stronghold, if not, indeed, the principal centre of the *Maetae*'. The other scholar argued thus: 'Dumyat and Myot Hill, if they preserve the name of the *Maetae*, may help to fix the territorial limits of that people. It is highly improbable that as names they were given to these places by the *Maetae* themselves, if only because peoples do not usually give their own names to their own fortresses, however important they may be. We should assume that the name Dumyat, 'fortress of the *Maetae*', arose first in the speech of a neighbouring people to whom it was familiar as a landmark or by report. If the name *Maetae* occurs in the names Dumyat and Myot Hill, therefore, the conclusion should be, not that those places were especially important (though they might have been), but that they were near the boundary which separated the *Maetae* from their neighbours in whose speech the two names arose. In point of fact, there is good reason to believe that Dumyat and Myot Hill stood within a few miles of the southern boundary of the *Maetae*'. The two sets of assumptions may be compared. And anyone who drives towards Stirling from Edinburgh, Glasgow or the further south may amuse himself by noting his position when the impressive mass of Dumyat forces itself upon his attention.

The first of the two scholars referred to above had in mind a similar name, Dumbarton, 'the fortress of the Britons', and he regarded it as a good parallel to Dumyat. It is a good parallel, but not necessarily in the way he saw it. Dumbarton, too, is a name given to a fortress by a neighbouring people, in this case by the Scots of Dalriada, to whom it was a prominent landmark on the northern confines of British territory, almost an outpost thrusting forwards into an area that was geographically part of Dalriada not of Strathclyde. It also happens to have been the chief fortress or capital of the Britons of Strathclyde. But the name Dumbarton (*Dùn Breatann*) was given to it by the neighbouring Scots. We happen to know that the Britons themselves called it *Alclut*, 'the rock of Clyde'. As urged above, a people do not usually give their own name to one of their own fortresses, even their capital. In any case the Britons of Strathclyde called themselves *Gymry*, 'fellow-countrymen', like their kinsmen in Wales.

VI

SCANDINAVIANS

STUDENTS of the Scandinavian settlements in Britain have long recognized that place-names may be used to expand and amplify the historical record. The first essential, as always, is to get a clear impression of the names linguistically, for this controls all interpretation of them in terms of history. It is important to know, for example, if the names under review are strictly Scandinavian compounds, often preserving such Scandinavian inflexional forms as the genitival *-ar*, *-a* and *-s*,¹ or if they were originally English names Scandinavianized by sound-substitution or word-substitution.² It is not always possible, of course, to be sure of the original form of a name, or to distinguish

¹ E.g. *Almunderby* YW, 1086 *Almundrebi*, ON *Álsmundarbyr* (*Álsmunde*); *Scalby* YN, 1086 *Skallebi*, ON *Skallabýr* (*Skalli*); *Braceby* LI, 1086 *Bre(i)gbi*, ON *Breiðbýr* (*Breiðr*).

² E.g. *sh* for *th* in *Scalford* (OE *sc(e)ald*, 'shallow') and *Skipton* (OE *scip*, *scip*, 'sheep'); *k* for *ch* in *Keswick* (OE *cāse*, 'cheese') and *Kilwick* (OE *cild*, 'child'); ON *mydal* for OE *middel*, 'middle', in *Melton* (Melton < *Maelton* < *Medaltūn* < *Middelūn*); ON *konungr* for OE *cyning*, 'king', in *Coniston* (*Konungstūn* < *Cyningstūn*), etc.

between sound-substitution and word-substitution, and allowance must also be made for the Anglicization of Scandinavian names and of English names earlier Scandinavianized. Leaving on one side the many difficulties of this kind, however, distinctions such as those mentioned above are significant for the historian. It is clear, for example, that strictly Scandinavian compounds, especially those which preserve traces of grammatical inflexions, imply that in an area where they are found the spoken Scandinavian language had lost little or nothing of its vitality at the time when they arose. In these areas it is permissible to assume the existence of a very strong Scandinavian element in the local population. Scandinavianized forms of earlier place-names also indicate strong Scandinavian linguistic influences, and a considerable number of Scandinavian-speakers must be assumed to account for them.

It is all very difficult, however, for what the names measure is linguistic influence, and this is not necessarily a sure guide to density of population. In a thinly populated area a mere dozen or so Scandinavians might produce an overwhelming linguistic superiority, whereas several hundreds might make a far less noticeable impression on the place-nomenclature of an area already thickly populated by Englishmen. It is important to remember that place-names usually arise in the mouths of neighbours. Therefore even one strictly Scandinavian place-name or one heavily Scandinavianized English place-name ought to imply a comparatively strong Scandinavian linguistic influence in the area. A single Scandinavian settler would probably not affect the place-nomenclature of an area at all or, at best, his presence

might be commemorated by an English place-name such as Grimscothe which would simply refer (in English) to the cottage (OE *cot*) of a man who happened to bear the Scandinavian personal name Grim. Quite different are the implications behind a name like Grimsby (a Scandinavian compound) or even a name like Osmotherley (which, despite its second element, OE *liab*, 'clearing', has for its first element the Scandinavian personal name *Ásmundr* with an *-ar* genitival inflexion). Similarly a considerable Scandinavian element in the local population would be required to change an English 'Shaldford' to Scal-ford, an English 'Shipton' to Skipton and an English 'Cheswick' to Kerwick. Many considerations such as these must be taken into account before one accepts the validity of assumptions underlying historical interpretations.

As it is necessary to pay attention to the chronology of English place-name patterns in order to use them effectively for the study of the Anglo-Saxon settlement, so it is necessary to obtain a clear idea of the chronological range of Scandinavian place-names if one proposes to use them to throw light on the extent and progress of the Scandinavian settlement. It is still a common practice to fling all 'Scandinavian' place-names on a blank map, irrespective of their age and significance, and treat the result as though it gives a picture of several unrelated aspects of the settlement. This is quite illogical. It is more satisfactory if, as on a map recently published by the English Place-name Society, only parish-names of Scandinavian origin are used on a distribution map to illustrate the Scandinavian settlement, but even this involves large and unjustifiable assumptions. What we need is a complete list of place-names which arose between 875 and

925. Unfortunately it is at present impossible to compile such a list, for a great many Scandinavian place-names could have arisen at any date between 875 and 1066, and not a few could have arisen at any date between 875 and 1250. It is possible, however, to advance some distance towards the goal and to indicate certain patterns of place-nomenclature which seem to be closely associated with the first phase of the Scandinavian settlement.

An investigation undertaken some years ago seems to show that, with important qualifications, place-names which have *ODan by* as their final element may be used as a *class* to indicate areas of heavy Danish settlement in England during the last quarter of the ninth century and the first quarter of the tenth.¹ There are over 760 place-names in *-by* (ON *býr*, *ODan by*, 'village, farmstead') recorded in England, and about three-quarters of them are proved by their inclusion in Domesday Book to have been in existence and to have been attached to substantial places before the end of the Old English period. Almost exactly two-thirds of them have a personal name as first element (e.g. *Eindriði* in Enderby, *Klakkr* in Claxby, *Malti* in Malthy, *Sumarliði* in Somerby and *Uglubarði* in Ugglebarnby) as distinct from a descriptive or non-personal word (e.g. *dalr* in Dalby, *griss* in Grisby and *saurr* in Sowerby). An examination of the personal names combined with *by* was designed to test their antiquity in so far as this is revealed by their occurrence independently in later times and by their occurrence with other and pre-

¹ The details and results of this investigation, which formed part of a Ph.D. thesis, have never been published. Perhaps they ought to be if only because it is obviously unsatisfactory to attempt to summarize them in a few sentences here.

sumably later place-name elements. Even before the investigation began it was clear that the personal names combined with *by* in eastern England present an unusually varied name-series in which is embedded a comparatively archaic stratum. This became more impressive as the investigation proceeded. In the end it was possible to say with some confidence that in areas of Danish influence place-names in *-by*, regarded as a class, very closely represent the first phase of the settlement and that most of them arose at a date which cannot be far removed from A.D. 900.

It should be emphasized that the above conclusion does not apply to areas of predominantly Norwegian settlement, for in these areas *byr* remained a productive place-name element until long after the Norman Conquest, as is shown by such Cumberland examples as Harraby (Henry), Johnby (John), Robberby (Robert) and Upperby (Hubert) which cannot have arisen before 1092 and probably belong to the twelfth century. Secondly, it must be accepted that even in Danish areas some place-names in *-by* probably arose at a date nearer to 1066 than to 875, but this does not seriously affect the validity of conclusions drawn from the names as a class. It should be remembered also that non-personal place-names in *-by*, a third of the total, were excluded from the chronological investigation because no direct means of testing the antiquity of their first elements could be found. This does not mean, however, that they are necessarily late formations. Indeed, if one were to judge from parallels in Scandinavia, one would accept them as early, especially those which have a topographical first element (such as *dalr* and *saurr* in the several examples of Dalby and Sowerby, *lindr* in Lumby, and *mór* in Moorby and Moreby), for these would be counted

as among the oldest of place-names in *-by* in Scandinavia.¹ The occurrence of elements such as 'cross' (OIr *cross*, ON *kross*, ME *cras*) in Crosby, 'kirk' (ON *Kirkja*) in Kirby and Kirkby and 'new' (ON *nýr*, OE *nīwe*) in the common Newby by no means destroys the impression of antiquity of place-names in *-by* as a class, not least because they appear most frequently in the Norwegian areas of the north and north-west where we know that *byr* remained alive and productive as a place-name element until a comparatively late date. Finally, it should be stressed that place-names in *-by* are not the only place-names that go back to the Scandinavian settlement. It is true that names like Grimston (on which see below, pp. 82-84) and the Danish 'thorpes' are by and large characteristic of secondary settlement, but in this context 'earlier' and 'later' are not mutually exclusive terms; they are indicative rather of chronological range or emphasis. An element current during, say, the period 850-1050, with most of its examples falling between 850 and 950, would be described as 'earlier' than an element current during a longer

¹ The element *by* has been much discussed by Scandinavian scholars, and its origin and development are still disputed, though there is substantial agreement that in Scandinavia it denotes secondary settlement or expansion from an older village. It is perfectly clear that non-personal words are much more common than personal names as first elements with *by* in Scandinavia, and that personal names were combined with *by* only at a comparatively late date, though the practice had begun before the Scandinavian settlements in England. These discussions are of fundamental importance, but it is doubtful how far they affect the interpretation of place-names in *-by* in England where, as noted above, examples with a personal name as first element are twice as common as those with a non-personal word. It is clear that in Danish areas a *by* is a 'village', which became a common meaning in Denmark and Sweden, as distinct from a 'farm' or 'isolated dwelling' which is still the common meaning in Norway and may be accepted in areas of Norwegian settlement in England.

period (say 850-1250) and similarly as 'earlier' than an element current during the same period (850-1050) but with most of its examples falling between 950 and 1050. There is no reason why names like Staxton (ON *Stakkr*) and Scunthorpe (ON *Skúma*) should not be as old as the oldest *by* in England, and in fact many such names must go back to the first phase of the settlement. The investigation referred to above does no more than seek to isolate an easily recognizable place-name pattern which, as a pattern, seems to be early and seems to be directly related to the first phase of the movement which, as we know from historical sources, established considerable settlements of Danes in Northumbria (876), eastern Mercia (877) and East Anglia (879-880).

It is not the purpose of this chapter to draw the conclusions that may be drawn from place-names, but simply to illustrate the approach, the requirements and the difficulties. Therefore we shall not here present the conclusions that may be drawn from place-names in *-by* and their distribution. To do that would, in any case, fill another book, for there is much more to it than the mere plotting of all examples of *by* on a map. By way of summary it can be said that place-names in *-by* indicate with some precision the boundaries of the areas settled by the Danes of the Great Army in 876-880, throw into prominence the areas most thickly occupied by them and, by their changing character, suggest areas where the Danes, though politically dominant, were subjected to the linguistic influences of English communities of considerable strength. By way of illustration attention may be drawn to the heavy concentrations in Lincolnshire and Leicestershire, especially in the Spilsby area of Lindsey (with some seventy examples like Aby, Ailby, Anderby, Asgarby,

etc.), in the Sleaford-Bourne areas of Kesteven (with some forty examples like Dowsby, Harrowby, Humby, Keisby, etc.), and in the Wreak Valley of Leicestershire where within five or six miles of Melton Mowbray (*Medallion*) are clustered some twenty-four examples (Ab Kettleby, Asfordby, Barsby, Brentingby, Freeby, Hobby, Rearsby, Rotherby, Somerby, etc.). In these three areas so great is the strength of Scandinavian linguistic influence that we must assume the English inhabitants to have been completely outnumbered and overwhelmed politically and socially as well as linguistically. And this conclusion depends not only on the concentrations of place-names in *-by* but also on their constituent elements which preserve many examples of grammatical inflexions, proving the existence of Scandinavian-speaking communities, and of personal names which by any test would seem to have become obsolete in England soon after the end of the ninth century. On the disputed question of how many Danes, absolutely as distinct from relatively, settled in England it should be borne in mind that in Danish England a *by* was a 'village', not a 'farm', which means that we should count the newcomers by the thousand rather than by the hundred. As for the character of the settlement, as revealed by place-names, it seems to have been the settlement of an army on the ground. In many cases the personal names combined with *by* are no doubt the names of the subsidiary leaders of the so-called Great Army which formally occupied and settled in eastern England during the years 876-880. Not only the evidence of place-names points to this conclusion.

The chronological investigation referred to above also threw light on what may be called the 'Grimstoo hybrids'. These are place-names in which a Scandinavian personal

name is combined with OE *tun* ('village'), e.g. ON *Grimr* (ODan *Grim*) in Grimston, *Fótr* (*Fot*) in Foston, *Krókr* (*Krok*) in Croxton, *þorketill* (*Thorkeitel*) in Thurstaston, *þorsteinn* (*Thursten*) in Thrussington, etc. The personal names in question are 'later' than those combined with *by*, but only in the sense that a smaller proportion of them had apparently become obsolete within a few years of A.D. 900—or in the sense that a larger proportion of them remained alive for a longer period in independent use and in combination with other place-name elements. While many of them no doubt go back to the first phase of the Scandinavian settlement, it is clear that *as a class* they mark a secondary phase of settlement in the sense that it overlapped and lasted longer than the first phase.

What is perhaps even more interesting is their distribution. They are not found in or among the most impressive concentrations of place-names in *-by*, but they are found in areas outside or marginal to these concentrations. They are not found in the Spilsby, Sleaford and Wreak Valley areas, for example, but on the fringes of these areas; they are also common in Nottinghamshire and Derbyshire, where place-names in *-by* are comparatively few, and they are most common of all in Norfolk and Suffolk. Now 'Grimston bybrids' are English place-names, of course, and the immediate implication is that they indicate Danish settlement in areas where English linguistic influence was strong enough to hold its own. They do not imply that Danish settlers were few in Nottinghamshire, Derbyshire and East Anglia, but that the English element in the population was comparatively strong. The English may have been overwhelmed politically in these areas, but they were numerous enough to hold the balance linguistically. Conditions must have been very different in

the Wreak Valley. In many cases the Scandinavian personal names combined with *tan*, like many of the personal names combined with *by*, may be the names of subordinate leaders of the Great Army who settled on the ground with their followers in the years 876-880. The 'Grimston hybrids' may often be the new names of English villages taken over by the Danes. But if a new name was Grimston instead of Grimbsy the implication is that the Danes were settling in a countryside which remained peopled largely by Englishmen. The conclusions that may be drawn from such distributions are self-evident. They throw much new light on the Scandinavian settlement, not least on the Scandinavian settlement of East Anglia which has long presented problems to the historian.

Much emphasis has been laid on the importance of trying to construct chronologies of place-name types and patterns, and if the intention is to draw conclusions about a particular period it is clear that place-names which arose long after that period are not what the historian would call contemporary evidence. And it is true that the evidence of contemporary place-names, that is of place-names which arose or developed during the period concerned, is the most reliable and the most instructive kind of evidence, especially if detailed conclusions of a chronological character are sought. Perhaps it would be more accurate to say that the assumptions that can be made about them admit conclusions of a kind that cannot legitimately be drawn from place-names which arose in a later age. But convincing conclusions of a general character can be drawn from place-names of a later age, and often they are as important as they are convincing.

An example may be taken from the Northern Isles. For

this purpose we may leave on one side recent attempts to evolve a chronology of place-name patterns in Orkney and consider only the general picture presented by the place-nomenclature of the Northern Isles. It is an impressive picture. Apart from an insignificant number of later Gaelic and English introductions, it is a picture of intensive and ubiquitous Scandinavian influence. Without long lists of place-names, or at least of elements, it would be quite impossible to indicate its thoroughly Scandinavian character. Scandinavian names—of hamlets, hills, ravines, rocks, nesses and voes, farms, fields, streams, mounds, banks and enclosures—run into thousands, and they wrap the islands in a thick and distinctively Scandinavian blanket. Even if we did not know that a Scandinavian language was spoken in the Northern Isles until about two hundred years ago, we could account for such an overwhelming linguistic influence only by assuming the occurrence at some date of an immigration sufficient to wipe out almost every linguistic trace of earlier peoples. Such an immigration took place at the beginning of the ninth century and not, so far as we can see, at any subsequent date. Therefore we are entitled to assume, on the evidence of place-names growing and developing over a thousand years, that the settlement which took place early in the ninth century had the force of a mass-migration. It will be noticed that the evidence on which this conclusion is based is the whole body of developing place-nomenclature, not the comparatively few place-names whose origin can be traced back to the period of the settlement. It is a matter of making sure that the conclusions proposed are such as do not involve assumptions which the evidence cannot support. Conclusions which have a chronological content must be based on evidence

is an exact chronological context, as a rule, but the same limitation does not necessarily apply to conclusions of a more general kind.

A similar example may be offered from England. The Leicestershire village of Hoby stands on the western edge of that remarkable concentration of place-names in *-by* which has already been noticed in the Wreak Valley (see above, pp. 82-84). As one would expect, its minor names are very strongly Scandinavian in character, and from a document of 1322 the following examples may be quoted: *Le Holm, Gamelisholm, Ribolm, Scrapholm, Swinbolm, Tasi-bolm, Thacholm, Ousterdale, Oustrenges, Fulwellegate, Melton-gate, Wodegate, Rischebuskes, Hamerrischebuskes, Otterwine-lisbuskes, Hamerwonges, Mikalwonges, Prestewonges and Thurbernewonges*. Before we attempt to draw any conclusions from these names let us pause and examine the validity of certain assumptions. It has been urged above that field-names and other minor names lack the wide acceptance that makes for permanence and so they are frequently changed or replaced (see above, p. 58). They are identificatory labels like all place-names (see above, pp. 10-11), but they are less divorced from ordinary language and there is a strong tendency for them to fall out of use when their meanings become obscure. A few minor names have remained in use for centuries, of course, but the great majority seem to have been changed from time to time. This tendency towards replacement is recognized by place-name scholars, for they are unable to trace the existence of more than a very small number of minor names over any considerable period of time. That the tendency springs from their impermanence is self-evident. That it has the result of keeping minor names more closely in line with the ordinary spoken language

will be recognized by all who give a moment's thought to any body of current minor names known to them. It will be noticed that people seldom understand the meanings of current place-names (i.e. major names or place-names proper) but they find few difficulties in the interpretation of current field-names. There are always one or two that have lost all meaning and are quite obscure to those who use them—these are the few 'archaic' names which have survived for a long time because their value as identificatory labels has given them a local permanence—but the overwhelming majority are names like *South Field*, *Church Field*, *Mill Field*, *Marled Field*, *Stony Field*, etc., and these present no problems of interpretation to those who use them. Is it a fair assumption then that the great majority of field-names, unlike place-names proper, fall out of use when their meanings become obscure?

Accepting for the moment this assumption as valid, let us see what conclusions we may draw from the above series of minor names in Hoby. We may conclude, if we do not know it already, that Scandinavian words like *bolmr* (ME *bolme*), 'island, water-meadow', *gata* (ME *gate*), 'way, road', *buskr* (ME *busk*), 'bush', *eng* (ME *eng > ing*), 'meadow, pasture' and *rangr* (ME *rong*), 'strip of meadow' had passed into Middle English and were part of the ordinary language current in Leicestershire in the fourteenth century. We may conclude too, that Scandinavian personal names like *Gamall* (*Gamel*) and *þorþiørn* (*Thurbern*) were alive and in use. We may go further and, relying on the validity of the assumption, conclude that the English word 'east' had not yet displaced the Scandinavian *austr* in the local dialect. We may even conclude that the medial *e* in *Thurbernewanges* represents the old Scandinavian genitive *a*. Without pressing such details

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too far we may safely conclude that the men of Hoby in 1322 were still speaking a language that had much in common with the language of the Danes who had settled in the area four and a half centuries before. The minor names of Hoby carry us back to the Danish settlement and they testify, indirectly but none the less convincingly, to its scale and its intensity.

Many other considerations limit the scope of legitimate inference from the evidence of place-names. It is impossible even to mention them all, for they vary almost from name to name. The above examples are no more than examples, and each new conclusion requires a reappraisal of the assumptions involved. The fundamental fact to be kept in mind is that place-name evidence is linguistic evidence. Any non-linguistic information preserved by it is incidental and can be revealed only by indirect inference. Non-linguistic conclusions, which include all the conclusions normally of interest to the historian, must depend upon assumptions. Therefore it is important to make sure that one's assumptions are sound. Or at least to recognize and allow for their weaknesses. Tentative conclusions are better than no conclusions at all so long as their nature is understood.

VII

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IN the preceding chapters attention has been focused on the difficulties encountered by those who seek to use historical, archaeological and linguistic evidence. There are the difficulties encountered in its recognition and collection, the difficulties encountered in its interpretation in terms consistent with itself and, finally, the formidable difficulties encountered in the co-ordination of results into a single synthesis. The fundamental difficulty arises from the fact that historical evidence, archaeological evidence and linguistic evidence are essentially different in their nature. From this it follows that they require different methods of approach and are subject to different sets of limitations. They also produce conclusions fundamentally different in character. To bring these different conclusions together into a single synthesis, commonly called a historical synthesis, is a very difficult business. It is not surprising that attempts at co-ordination produce conflicts, strains and uneasy balances.

There is one kind of co-ordination that is at once

direct and effective, but it is simple co-operation rather than co-ordination on the scale envisaged above. There are many instances of help given to each other by historians, archaeologists and place-name scholars. Sites mentioned in the written record but lost to the historian may be identified by the evidence of place-names and more fully explained by the evidence of archaeology. Linguistic criteria embedded in a copy of a charter or a manuscript of a chronicle will often help the historian to determine the authenticity of his materials by revealing the date of underlying sources. Certain linguistic forms in the surviving manuscripts of the Anglo-Saxon Chronicle, for example, demonstrate that the ninth-century compilation was in part based upon otherwise unknown annals which were in writing early in the eighth century. More spectacular, perhaps, are the linguistic indications that the late and only surviving manuscript of the *Three Fragments of Irish Annals* incorporates sources which were already in writing in the ninth and tenth centuries, over seven hundred years before the date at which the manuscript was transcribed (see above, pp. 16-17). In return the historian assists the linguist by providing him with reliable and dated material for his studies. He also assists the archaeologist by providing him with historical background for his material, thus enabling him to advance to more elaborate and more accurate interpretations and, equally important, enabling him to address more significant questions to his material. Collaboration of this kind is of the greatest importance, and examples of its successful application now colour much of what is written about the Dark Ages. But it is co-ordination on a comparatively simple scale, and it does not raise problems of the same magnitude as attempts to integrate results on a broad

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front. It has much in common with the help that the archaeologist receives from natural and physical scientists. Co-operation is a more accurate description of it than co-ordination.

Even at this level co-ordination presents problems. Any specialist who has asked questions of another specialist or had questions addressed to him by another specialist will understand the basic difficulty. Questions which seem reasonable to the questioner are often regarded by the recipient as illogical or, at best, improperly phrased. And answers, though reasonable in themselves, are often unintelligible to the person who receives them or, even if they are intelligible, they miss the central point of the question. Exchanges of question and answer can be of great value at their most successful, of course, but at the other end of the scale we all know how irritating both questions and answers can be. The fact of the matter is that the specialist who asks questions and the specialist who answers them are both to a great extent beating the air unless each understands the other's problems. And to understand the other's problems presupposes an appreciation not only of the nature of his evidence but also of its limitations and of the methods and techniques by which it is studied.

The fact that the evidential materials for the study of history, archaeology and place-names respectively are essentially different in character has been sufficiently emphasized in the preceding chapters. It is enough here to repeat that historical evidence is written evidence and direct evidence only of the state of mind of the person who composed it or dictated its composition, that archaeological evidence is essentially material evidence

and direct evidence only of practical skills, technological processes, aesthetic interests and physical sequences, and that place-name evidence is essentially linguistic evidence and direct evidence only of language and speech habits. To assemble the relevant evidence in each case and then to interpret it within its own proper sphere, that is as direct evidence, are operations that pose problems enough for any scholar. But they do not reach formidable proportions so long as the limits of direct inference are not exceeded, that is so long as the archaeologist seeks only material and physical conclusions, so long as the place-name scholar seeks only linguistic conclusions, and so long as the historian does not stray outside the bounds imposed by the nature of the written record.

Greater problems of interpretation arise as soon as specialists advance outside the narrow confines of their own disciplines. But one cannot expect the archaeologist to contain himself rigidly within the limits of direct inference, and it is not desirable that he should. He realizes that the potential information preserved in his material goes far beyond comparatively simple conclusions about how pots were made, how hill-forts were constructed, to what extent physical appearances were important and in what order physical sequences occurred. He knows that it offers him many reliable conclusions of an economic nature, and not unnaturally he wishes to reach out towards possible political and social implications, and even to allow himself an occasional comment on motives and intentions. Similarly the place-name scholar sees that his material will yield more than conclusions about linguistic developments, and so he presses forward into the non-linguistic world of political movements, social conditions and economic change. There is no good reason why both

the archaeologist and the place-name scholar should not thus venture outwards, for these aspects of human activity are often reflected in archaeological and linguistic material. It is true that the reflections are often so faint that they have less form than a will-o'-the-wisp, but only the narrowest of pedants would turn his back on them altogether. It is in these extra-liminal excursions, however, that the worst hazards lie. They are always dependent upon assumptions, assumptions that become more and more strained as the distance between the material and the conclusion is increased. Enough has been written about assumptions in the preceding chapters. They range from reasonable propositions capable of supporting the burden of inference to subjective fancies and extravagant absurdities capable of supporting nothing but fantasy. Every assumption must be examined carefully, for on its validity depends the reliability of all conclusions based upon it.

Even greater difficulties arise when a scholar, having stretched out beyond the limits of direct inference in his own field, then attempts to relate his conclusions to evidence in another field. This is the beginning of co-ordination proper, but it is useless unless it is based upon an understanding of the nature and limitations of both kinds of evidence. Some archaeologists—one must say this—frequently adopt a cavalier attitude towards historical evidence. They are too much given to picking up a chronicle, often in an out-of-date edition or a faulty translation, abstracting an isolated entry from it, and then treating it as though it were as inherently uncomplicated as a piece of pot. They would do well to bear in mind the problems presented by historical evidence as they are summarized above (see Chapter II). An

entry in a chronicle is not the same thing as a historical fact.

The annal for 571 in the Anglo-Saxon Chronicle has been quoted before in this context, but it will serve as an example as well as any other historical statement. In the library of Corpus Christi College, Cambridge, the oldest surviving manuscript of the Anglo-Saxon Chronicle contains an entry which records that in 571 a certain Cuthwulf fought against the Britons at 'Bedcanforda' and captured the four towns (*townas*) of Limbury, Aylesbury, Bensington and Eynsham. Similar entries occur in manuscripts in the British Museum and in a manuscript in the Bodleian Library—in the last Cuthwulf or Cuthulf is called Cutha. We are not automatically entitled to assume that in 571 the West Saxons advanced for the first time into territory held by the Britons. We are not entitled to assume that Cuthwulf's men were West Saxons, despite the alliteration of his name with other names in the West Saxon royal family and despite the association of a Cutha with Ceawlin in an earlier annal. We are not entitled to assume that 'B(i)edcanforda' is Bedford—indeed we cannot assume this identification in the face of the linguistic evidence unless we also assume that the form is seriously distorted in all the surviving manuscripts of the Anglo-Saxon Chronicle (or rather in the common ninth-century stock from which they are derived). The possibility that the annal is displaced has been suggested, and we are not even entitled to assume that Cuthwulf fought a battle against the Britons in 571. If it comes to that, we are not even entitled to assume that he ever existed outside some early legend current among the English. The location of an entry in a chronicle is only the beginning, not the end, of a complicated investigation which, if it is successful,

may produce a reliable historical conclusion. And even that can never have the precision of a mathematical fact or the simple reality of a piece of pottery.

Archaeologists would do well to recognize the complexity of historical evidence. They are altogether too ready to seize upon a snippet from the historical record, divorce it from its proper context, divest it of all doubts and uncertainties, and then relate it triumphantly to some conclusion of their own. They are too ready, also, to dismiss historical evidence if it does not accord with their own conclusions. The difficult annal for 571 has been dismissed by more than one archaeologist, and some would discard all the early annals of the Anglo-Saxon Chronicle as an uncritical reconstruction of legendary events and incidents. This will not do. If the literary traditions preserved in the Anglo-Saxon Chronicle are to be discarded, it must be on the arguments of historians and as the result of a historical examination. It would be ludicrous to dismiss historical evidence simply because it is held to be contradicted by non-archaeological conclusions drawn by an archaeologist from archaeological material. There is no point of contact between the historical evidence and the archaeological evidence itself, and so they cannot be in conflict with each other. Historical conclusions based on one may be in conflict with conclusions based on the other, in which case either or both sets of conclusions may be wrong, as they have often been in the past. But it is surely not permissible for an archaeologist to dismiss historical evidence, the nature of which he has not taken the trouble to understand, by opposing against it not, indeed, straightforward archaeological evidence but merely his own conclusions based on archaeological evidence, especially when dubious assumptions loom larger

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in his conclusions than does the archaeological evidence itself.

It should not be thought that archaeologists have a monopoly in the mishandling of evidence. Historians frequently misuse archaeological and linguistic evidence, either by ignoring the assumptions underlying the inferences based on it or by mistaking the conclusions for the evidence itself. Some of them have a touching faith in the written conclusions of archaeologists and place-name scholars, swallowing them entire with no regard for the canons of criticism which they have been trained to observe. Others are equally uncritical in their condemnation of all archaeological and place-name studies, an attitude which puts them in the same category as that of the archaeologist who dismisses historical evidence without understanding it. Some historians even mishandle historical evidence, and for that there is less excuse.

Even the austere race of philologists is not beyond reproach. Some of them, deluded by the precision attainable in their own studies and forgetful of the nature of other kinds of evidence, regard historians as blunderers and archaeologists as charlatans. Some of them, on the other hand, though critical and cautious in the handling of their own material, seem to think that historical evidence can be manoeuvred to suit any argument. And one of them, a continental scholar of international repute, recently entered the archaeological field to emerge with an utterly fantastic series of reconstructions, apparently oblivious of the fact that you can produce any pattern you like if you rely entirely on assumptions drawn from an unrestrained imagination.

The views and attitudes summarized in the last five or

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six paragraphs must not be attributed to all archacologists, historians and philologists. That would be absurd. Most scholars fully recognize the nature and limitations of evidence and, despite occasional exceptions, the most distinguished scholars are also the most careful in their use of it. The most extreme cases of evidence misunderstood and misused are found among amateurs and popular writers. In an age when books on history, archacology and place-names are attracting ever-widening circles of readers true scholarship is in danger of being swamped by cheap journalism, and one increasingly hears the most unlikely names quoted as authorities. There is no law to protect scholarship from the ill-effects of publicity, and the purveyors of rotten meat rush in to feed the hungry public. There is a danger that the follies of the second-rate may come to obscure the entirely different approach of the serious scholar. The less extreme cases of the misuse of evidence do not so readily catch the eye, but they are more common among scholars as distinct from the hangers-on of scholarship. At all levels the same obligation applies: if anyone elects to wander into the field of a specialist he should make himself familiar with the nature of the evidence that he proposes to handle and especially with the limitations on inference imposed by it. If he is unwilling or unable to do this he would be wise to stay outside the fence.

However much one may play it down, there seems to be an inherent antagonism between scholars trained in different disciplines. It cannot be explained away as arising from unfamiliarity with a different kind of evidence or from annoyance when a specialist in one field misuses evidence that properly belongs to another. It goes deeper

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than that. Among historians, archaeologists and place-name scholars it seldom breaks out into denunciations and violent abuse, but it lies behind many a caustic comment, many an artless question and many a genial thrust. Fundamentally, of course, it stems from the differences in the nature of the evidence to which each is accustomed, but it is strengthened and barbed by concurrent differences in method. Some times it is a matter of ignorance only. The philologist, sitting quietly in his study and fortified by the knowledge that he can bring a battery of linguistic information to bear on his own evidence, may easily underestimate the difficulties of an excavator harassed by lack of time, lack of money, insufficient or incompetent assistance, bad weather, the sudden emergence of the unexpected and all the disadvantages of carrying out a scientific operation under conditions which a laboratory scientist would regard with horror. The philologist, confident in the precision of his own methods, may also fail to make due allowance for the complicated nature of the historian's evidence and for the correspondingly tentative nature of his conclusions. If he is tempted to criticize them, however, he does not get it all his own way. Archaeologists with mud on their boots and wind in their hair do not mince words. 'Until philologists put their house in order they can make no useful contribution to our studies', cries one. 'The study of place-names is not a respectable occupation for a scholar', solemnly declares another. Seldom do such forthright opinions find expression in print, but common-rooms and cocktail parties are frequently enlivened by even more prejudiced and colourful strictures.

But it is not always ignorance of the other specialist's methods and difficulties that underlies this mutual dis-

trust. Sometimes the methods themselves fall under the fire of cogent criticism, and unfortunately it is true that some scholars do not pay sufficient attention to the limitations of their evidence and are apt to press from it conclusions that it will not support even within the comparatively narrow range of its immediate applicability. Historians occasionally approach their sources without observing the formalities of historical criticism and put forward conclusions which do not rest securely on their evidence. Place-name scholars occasionally build too confidently on dubious parallels and insist on derivations which at best are speculative. The standards of excavation are probably higher in Britain than anywhere else in the world, but incompetent excavators, whose recorded observations are as suspect as their conclusions, are free to operate in numbers sufficient to bring the whole subject into disrepute in the eyes of many historians and philologists. Archaeologists who are fully aware of the complexity of modern excavation techniques occasionally fail to allow enough for the confusion produced by incompetent practitioners, and often they take on trust too much that ought to be subjected to a searching criticism. Frequently, too, in their own reports they are content to put forward conclusions whose only claim to acceptance is that they 'fit the observed facts'. It is not enough to put forward a conclusion on the grounds that it is not demonstrably wrong. Often there are several possible conclusions that 'fit the observed facts', and if one is to be singled out for acceptance we ought to be given the reasons for rejecting the others. It is a fair comment that archaeologists too often propose one of several possible conclusions for acceptance and invest it with a misleading semblance of reliability simply by passing over alterna-

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tives to it in silence. Faults such as these may be listed by the dozen. They are noted by other scholars, and they provide effective ammunition in the battle of criticism and counter-criticism.

The violence of these exchanges should not be exaggerated, for seldom are they ill-tempered and seldom are they uttered as reasoned pronouncements. It would be a good thing, perhaps, if scholars could be persuaded to raise their criticisms in serious discussions, those that are mistaken as well as those that are soundly based, for the questions at issue are of fundamental importance. Both critics and criticized would gain by having their difficulties brought into the open, and all would gain by an occasional reconsideration of the nature of evidence, the methods applied to it and the validity of conclusions drawn from it. This would be especially helpful in problems of co-ordination. It is only the conclusions, not the evidence itself, that can be co-ordinated, and therefore it is vital that conclusions should be sound or that their points of weakness should be recognized. An unsound conclusion vitiates co-ordination from the outset, and unsound conclusions arise from a failure to understand the nature and limitations of the evidence or from the application to it of inadequate or inappropriate methods. That is why both the evidence itself and the methods applied to it must be thoroughly understood. Despite their occasional criticisms of each other, archaeologists, historians and place-name scholars are not separated by insurmountable barriers. They are united by common aims and by a common respect for the sanctity of evidence. A fuller understanding of each other's materials would draw them closer together to the advantage of all.

The closeness of serious scholars to each other becomes

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at once apparent when one considers some of the mental aberrations which all combine to deplore. There are lunatic enthusiasts who locate archaeological sites of all kinds by drawing straight lines on maps, extract fantastic prophecies from measurements of the pyramids, trace Pictish influences in every scrap of evidence that comes before them, and create abstruse astronomical patterns from the marks on cup-and-ring stones. Their place is with the flat-earth theorists, and scholars either deplore them in silence or indicate the pathological nature of their afflictions by referring to them under such names as 'straightlinitis', 'pyramidiocy', 'pictomania' and 'cup-and-ringalucination'. It is an unnerving experience to talk to one of these mystical specialists. The amount of ingenuity displayed by them is astonishing: everything falls neatly into place, everything is explained, there is an answer to every objection, and after an hour or so one begins to feel some sympathy for the psychologist who mistook himself for his patient.

The study of these hallucinations belongs to the science of medicine, but certain aspects of them are of interest to the student of method. The victims are not lunatics in the ordinary sense of the word: they cross busy roads, drive cars, run businesses, discuss politics, exercise their civil rights and are accountable for their obligations in law. It is true that some of them ultimately cross the line of legal lunacy, but in most cases the obsession, for that is what it is, remains confined to one subject or one group of mental processes. It is a curious fact that many of the afflicted are men of considerable technical ability, often men who have devoted many years to the examination of evidence, especially evidence which demands careful observation and accurate calculation. Retired scientists

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and mathematicians seem to be especially vulnerable. How do they come to delude themselves?

It requires an effort of will to follow them into their lands of make-belief, but the light begins to dawn when one realizes that there is always a master plan, a unity which embraces and explains everything. It is as if they had taken some fantastic proposition into a closed cell with the single-minded determination to prove its validity by interpreting all evidence and framing all assumptions to that end. It is not really a difficult thing to do if you enter into the spirit of the game. The key is to work backwards from the preconceived conclusion. It is not for a moment suggested that scientists in their laboratories play games of this kind! But experiments in laboratories often achieve a remarkable degree of precision because they are devised to answer specific questions of limited range, because the information required for a conclusion is provided and because the relevant factors are known and controlled. In studies involving human activity, especially human activity that lies in the distant past, the evidence is entirely different in character. No experiment can be devised to revive the relevant conditions, which must remain to a great extent unknown, the human element greatly increases the range of possible explanations, most of the evidence required for a conclusion is irrecoverable, and much that is recovered remains obscure in its significance. Conclusions, therefore, are so tentative that a scientist might be forgiven if he regarded them as too vague to have any value. We are accustomed to accepting such limitations in our studies. But if a scientist, turning to history or archaeology, ignores these limitations, misunderstands the nature of evidence unfamiliar to him, applies to it the completely

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inappropriate methods devised for the laboratory sciences and seeks to extract from it conclusions which it cannot be expected to provide, then he is well on the way towards pathological absurdities like those mentioned above.

This excursion into the underworld of lunatic obsessions is not without its point, for it throws dramatic emphasis on what happens when evidence is used with no regard for its nature and its limitations. Ill-founded conclusions put forward by scholars, though less dramatic and far less outrageous, also arise from a failure to understand the nature of evidence and from the application to it of inadequate or inappropriate methods. It is important to remember that historical evidence directly represents only a state of mind and is therefore exceedingly difficult to disentangle, that archaeological evidence directly represents only certain aspects of the material world and that its recovery by excavation poses many complicated problems, that place-name evidence directly represents only linguistic developments and that its interpretation requires an equipment which cannot easily be explained to those who know nothing about it, that the frame of reference for the evidence available can never be fully known, that allowance must be made for evidence which cannot be recovered, that indirect inferences always depend upon assumptions, and that at best conclusions cannot be more than an approximation to the truth. It is from failure to remember points such as these that most of the conflicts of opinion among scholars arise.

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DISAGREEMENT about the interpretation of evidence and about the validity of conclusions that may be drawn from it is only one aspect of the problem of co-ordination. If all such conflict of opinion among scholars could be removed, there would still remain the conflict of conceptions. The past speaks with many voices—anthropology, ethnology, folk-lore and legend as well as history, archaeology and language—and, as the evidence offered by each approach to the past is distinctively different in character from the evidence offered by other approaches, so the conclusions based on these different kinds of evidence belong to entirely different conceptions. The nature of the evidence controls the nature of the conception. The historian, for example, writes of Picts, Scots, West Saxons, Northumbrians and Normans, and in doing so he is thinking of them in terms of historical evidence, which brings them before him as groups of people recognizable as more or less artificial organizations of human beings—

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in associations based on such ties as those of politics, geography, proximity, tradition and culture. If the ties are strong and well-developed he will call them nations; if they are not he will find some less definite name for them. He is not concerned with factors of race, language or material culture except in so far as these have contributed to the creation of the groups which he recognizes. The key to the real but intangible conception of a historical people is simply its appearance in the historical record. The archaeologist, if he writes of a people, is compelled by his evidence to think of them within the frame of a different conception: to him they are brooch-builders, souterrain-builders, beaker-folk, symbol-stone-carvers, saucer-brooch-wearers and similar groups of people characterized not by criteria of race, language or political divisions but simply by aspects of their material culture as represented by their buildings, pottery, ornaments, weapons, burial practices and the like. The philologist's conceptions are similarly controlled by the nature of his evidence: he sees men only in terms of language as Indo-European, Celtic, Brittonic, Gallo-Brittonic, Goidelic, English-speaking, Welsh-speaking, Danish-speaking and so on.

There is no point of contact between two different kinds of evidence and there can be no point of contact between the two conceptions to which they belong. They can never be equated in the sense of being identified with each other. They can be co-ordinated or, if one prefers the expression, equated in the limited sense of being brought into such close association that they illustrate the same idea, but always either different aspects of it or from different angles. If the final synthesis, the results of co-ordination, is commonly called a historical synthesis

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this is a usage which reflects the wider range of ideas embraced by historical evidence; it does not mean that archaeological and linguistic conceptions can be transmuted into historical conceptions. Like the evidence which controls their character they are separate and must remain separate. Conclusions of a political character, for example, which may be derived most naturally from historical evidence, may also be drawn from archaeological material and from place-names, but only by a series of indirect inferences which should never be allowed to obscure the fact that, being based on different kinds of evidence, they belong to conceptions fundamentally different from each other and from historical conclusions.

There is sometimes an appearance of similarity, but this is misleading, for it is conferred by the assumptions which have been combined with the evidence to produce the conclusions, not by the evidence itself. It is also sometimes suggested that archaeological and linguistic evidence are equated when inscriptions are found on stones or coins. This is not so. Inscriptions on stone, statements on paper and place-name forms on either stone or paper are merely examples of evidence in different contexts; this is always of interest, for the evidence and the context often throw light upon each other, but in every case the evidence itself and the conception to which it belongs are distinct from and independent of the context. Evidence, conclusions and conceptions can often be co-ordinated or brought into close association with other kinds of evidence, conclusions and conceptions. They cannot be equated or identified with them.

As different conceptions cannot formally be identified, so also they cannot formally be in conflict with each other.

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Mistaken interpretations and faulty conclusions may be in conflict with each other, but that is another matter. And it does not follow that apparent conflicts are always due to faulty conclusions. Evidence, conclusions and conceptions respectively may often be co-ordinated to produce remarkably coherent syntheses, but there is no necessary correlation. Race, language, nation and culture represent quite different conceptions, and it would be absurd to expect political boundaries always to coincide with ethnological boundaries, linguistic boundaries and cultural boundaries. The Picts, for example, are a historical people and the political boundaries of their kingdom are known, but it does not follow that they were a single race, that they spoke only one language, that they possessed only one material culture, and that the boundaries of these racial, linguistic and archaeological patterns were co-terminous with the political boundaries of Pictland. The true picture seems to have been quite different. We must be prepared to find that political, racial, linguistic and archaeological boundaries often do not coincide. This does not mean that the several conceptions are in conflict. Only that they are different. The picture that we are trying to recover is more complicated than it once appeared to be, but the prospect before us is more exciting, and there is a good chance that our synthesis will be nearer the truth.

The past is littered with mistaken attempts to equate historical, archaeological and linguistic conceptions. Even the last twenty-five or thirty years can produce a lamentable number of examples. The Picts have been equated with the broch-builders, an example which betrays a failure to realize that the two conceptions belong to different worlds and at the same time a failure to

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appreciate the true nature of the historian's conception of the Picts and the true nature of what is (or ought to be) the archaeologist's conception of the men who built the brochs. The Picts have also been equated with several other archaeological peoples, usually with archaeological peoples who had disappeared long before the Picts became a historical people, and these equations involve chronological contradictions as well as conceptual absurdities. There have been references to 'Goedelic beaker-folk' and 'Goedelic urn-folk', and philologists have even used the meaningless phrase 'Palaeolithic speech'. Examples of similar equations abound. In an earlier age such thoughtless or assumed equations were even more common, and we meet many terms like 'Pictish towers' (for brochs). All are equally illogical. But even worse than the formal absurdity of such equations, perhaps, is the fact that they have misled many later scholars into accepting associations which are not supported by the evidence.

Today the hazards, not to say lack of logic, in such equations are becoming more widely recognized. Within recent years a distinguished philologist has urged that 'the attempt to identify the language of a people whom we know only as an archaeological culture is a hazardous affair' and that it is not permissible to 'infer that if in prehistoric times a given culture is found in a given area, and no subsequent immigrant culture can be traced there, then the language spoken in that area when it first appears in historical sources was directly descended from that spoken by the people of the culture in question'. It has also been emphasized 'that political groups as recognized by the historian do not in themselves imply racial groups as recognized by the ethnologist, and that neither political

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groups or racial groups necessarily coincide with the cultural and linguistic divisions of the archaeologist and the philologist'. These admonitory pronouncements are, if one may say so, entirely sound and deserve the attention of all who propose to draw non-linguistic conclusions from place-name evidence and non-material conclusions from archaeological evidence.

The common fault today, however, springs not so much from deliberate attempts to equate or co-ordinate conceptions, for these are generally more circumspect than those of the past, but rather from casual or unconscious but equally uncritical equations. These, unfortunately, are still common in arguments, and it is no defence of them to say that their authors are unaware that they are assuming equations. Sometimes they take the form of opposing one conception against another as though they generate conflicts which can be resolved by discarding or modifying one of them, as in the example quoted by some archaeologists and the Anglo-Saxon Chronicle (see above, pp. 95-96). More often they take the form of casually transferring evidence and conclusions from one conception to another without realizing that large assumptions are involved.

The hazards are greatly increased by confusions in terminology, for these sometimes seem to imply equations where none exist. It has been pointed out above (pp. 104-5) that the historian, the archaeologist and the philologist use different terms to express ideas within the different conceptions by which they are bound. Unfortunately these terms are not always orthographically limited to use within one conception only and, even when they are, some scholars persist in misusing them. One of

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the clearest examples of the confusion that results is provided by the word Celtic, which has a specifically linguistic connotation and is properly used only when it describes language or linguistic features. It covers a group of languages which are known at different stages of development by such names as Brittonic, Gallo-Brittonic, Goedelic, Old Irish, Gaelic, Manx, Welsh, Breton, Cornish, etc.; but it covers nothing else. By definition a Celt is a person who speaks Celtic—no more and no less. Historians and archaeologists are not precluded from describing a people as Celtic, but only if they mean that the people in question is Celtic-speaking. It is wrong to use the word Celtic in a political or racial sense, and it is absurd to speak of an object as Celtic. Yet by daily repetition we have grown accustomed to Celtic brooches, Celtic art, Celtic churches, Celtic decoration, Celtic customs, Celtic villages, Celtic field-systems and the rest. They are all examples of a linguistic term transferred by careless usage to entirely inappropriate conceptions.

Even if we could turn back the clock and restore the word Celtic to its proper and exclusively linguistic setting, there would still be hundreds of other words which historians, archaeologists and philologists would have to reconcile themselves to sharing. Words like English, Scottish, Pictish, Danish, Norwegian, Scandinavian, British, Welsh, Irish, Northumbrian, Mercian and West Saxon spring to mind as examples of the many that are not restricted to a single conception. It might have been less confusing if scholars had agreed to coin and keep for themselves words to express their own ideas, but at this late date we must accept the various connotations of individual words and make the best of these rather inadequate vehicles for thought. The same problem has been

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met before (see above, pp. 6, 12-14). It is of no great consequence so long as scholars do not forget the original or intended connotations of the terms they use and so long as they do not allow themselves to be beguiled into basing conclusions on equations which are unintended or unproved—and sometimes even unnoticed. Unfortunately self-delusion of this kind is of very frequent occurrence.

Examples of it may be collected as easily as pebbles on a beach. One will suffice. It occurred only a few hours ago, after the first paragraphs of this chapter had been written, and another will no doubt be given to the world before they return from the typist. An archaeologist in a lecture that will be published (with the following words removed or modified we may hope) said: 'If an archaeologist, speaking only from archaeological evidence, may venture a tentative suggestion, I would say that the language spoken by the Picts was Celtic'. His suggestion, though tentative, was not tentative enough. Speaking as an archaeologist and from archaeological evidence he was not competent to make any suggestion at all on this topic. He can stare at brooches as long as he likes but they will never speak Celtic to him and they will never tell him what language their wearers spoke. If he was basing his suggestion on another argument (and we trust that he was), it must have been something like this: another people in another area are known, by other evidence, to have spoken Celtic and to have worn the same kind of brooches as those found in the Pictish area, and therefore the Picts also spoke Celtic. In this case, of course, he would be speaking not from archaeological evidence only but from assumptions—and in particular from the very odd assumption that people who wear the same kind of

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brooches must speak the same language, distance and other factors notwithstanding. It should be emphasized that this is not an isolated example of the confusion of conceptions; indeed it is not so confused as most. They occur by the hundred. And they do ill service to those who are striving to reach genuine associations and reliable co-ordinations.

If one means to avoid the mistake of confusing conceptions that are different in character, it is important to have a clear idea of the nature of the various conceptions. Much has been written in the preceding chapters about the nature of historical, archaeological and linguistic evidence, and the difference between the corresponding conceptions have been sufficiently emphasized (see above, pp. 104-5). A few points remain to be considered, however, especially in connexion with historical evidence and conceptions, for it is in this field that comments are most open to debate. There are several different conceptions or definitions of history, and some of them have been mentioned above (pp. 6, 12-14). In this book, for good or ill, the argument is that 'history is the study of the past as it is revealed by the written record', from which it follows that a historical conception is one that is derived from and dependent on the written record. According to this view a historical people is one that appears in the written record or, to put it more positively, a people is a historical people only if (and only because) it is mentioned in the written record. This standpoint is debatable, of course, and it would probably not gain the approval of a majority of either British or continental archaeologists. Therefore it is necessary to examine briefly, very briefly, the views of archaeologists on the nature of history. These turn upon

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the question of the dividing line between history and prehistory.

The French restrict the word *préhistorique* to the period during which there was no written record anywhere in the world, and by this definition they divide history from prehistory at a fixed date (about 3000 B.C.) which is applicable to the whole world. British archaeologists generally regard 'the local arrival of literacy' as the dividing line, with the result that for them prehistory ends at different dates in different parts of the world. These two opposed concepts have been much elaborated, usually with the intention of reconciling them by introducing sub-divisions to cover the period (which varies from area to area) between the appearance of the first written record and the local arrival of literacy. From the Disney Chair of Archaeology in the University of Cambridge comes the eminently reasonable proposal that prehistory should be divided into 'primary prehistory' and 'secondary prehistory': the former is the prehistory that precedes all history (and in his study of it the prehistorian is primarily an archaeologist, though he may receive assistance from natural and physical scientists); the latter covers non-literate peoples in the succeeding period when some peoples are literate and some are not (and in his study of them the prehistorian, still primarily an archaeologist, has the additional advantage of historical information and inferences derived from the literate peoples). It is clearly possible to postulate a whole series of graded relationships between the literate and the non-literate peoples and to take into account both the degree of literacy among literate peoples and the varying effects of their proximity and influence on the non-literate peoples. An understanding of these relationships helps to clarify the nature of the

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evidence available to the prehistorian and it establishes what may be called 'degrees of prehistory'. Their importance is beyond dispute. But whether or not they throw any light on the dividing line between history and prehistory is another matter.

From the historian's point of view the basic criticism of the French conception of prehistory is that it puts the cart before the horse in that it accords a greater importance to the date at which a written record appears than to the fundamental fact that it has appeared. Surely the significant fact for the study of any area or people is not when written records appear but whether or not that area or people itself appears in a written record. It is this appearance in a written record that alone can give a historical existence to an area or a people. At least that is the view adopted in this book. It rejects the French definition of prehistory as putting the emphasis in the wrong place and as subordinating fundamental issues to superficial forms. It also clashes uncompromisingly with the views of British archaeologists in its insistence that the conception of a historical people depends upon its appearance in a written record, any written record, and has nothing to do with the local arrival of literacy in an area. A people need not be literate in order to qualify as a historical people by appearing in a written record. The Britons are a historical people because they appear in a written record, not because they appear in their own written record. And the Picts would still be a historical people even if not one of them had ever written a single word. It is the fact that they appear in a written record that makes them a historical people and gives them a historical existence.

So much for a historical conception within the definition of history as the term is generally used in this book,

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that is to cover the study of the past as it is revealed in the written record, as distinct from history used in the wider sense to cover 'all aspects of and all forms of inquiry into the human past' (see above, pp. 6, 12-14). The latter is the sense used in the phrase historical synthesis, meaning the synthesis to which historical conceptions proper as well as archaeological, linguistic and other conceptions all make their separate contributions. It has been sufficiently emphasized that these conceptions, each controlled by the nature of the evidence from which it is derived, must not be confused. Failure to observe this requirement opens the door to mistaken associations which render co-ordination ineffective.

The Picts, for example, are a historical people, and it is confusing as well as formally not permissible to project them backwards in time beyond A.D. 297, the date of their first appearance in the written record. The historical Picts are the only Picts known to us; the historian cannot know anything about them before A.D. 297; and the archaeologist cannot know anything about them at all unless he first manages to bring one or more of his archaeological conceptions into close association with them. This is far more difficult to do than it would seem. It has been possible to bring one group of souterrain-builders, those of east-central Scotland, into association with the Picts by demonstrating archaeologically that their descendants were one element in the agglomeration of peoples which after A.D. 297 was known historically as the Picts. But this is a complicated process, and in other contexts it has seldom been possible to carry it through to a successful conclusion. The reason for this lies in the barriers between historical and archaeological conceptions. It has recently been suggested, for example, that the

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broch-builders perhaps represent only the structural aspect of what in a historical conception might have to be represented as a gradual fusion of two or more peoples. If this comes anywhere near the truth then it would be as difficult to associate the broch-builders with any historical people as it would be to decide who were the castle-builders of twelfth-century England. Political and social implications are there in each case, but we should not be able to recognize and make sense of them if we had only archaeological evidence and conceptions to guide us.

Despite the difficulties always present in attempts at co-ordination, it is often possible to bring different conceptions together in convincing and reliable associations. The distribution of place-names indicative of early Anglo-Saxon settlement (see above, pp. 63-70), for example, produces a pattern which corroborates and is corroborated by the distribution of pagan cemeteries, as may be seen visually represented in Maps 1 and 2. There is more than mere corroboration in the result, for each amplifies the conclusions that may be drawn from the other and both add new and valuable definition to the details preserved in the historical record. Despite certain anomalies, on which more is said below, this synthesis of historical, archaeological and linguistic conceptions is an example of remarkably successful co-ordination. The distribution of place-names thought to be indicative of the first phase of Danish settlement (see above, pp. 72-82, and Map 3) throws a vivid and revealing light on the scope, scale and nature of a movement which, though of outstanding significance to the history of England, the historical sources themselves dismiss in brief and casual references to the fact that contingents of the Danish Great Army

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settled in Northumbria, Mercia and East Anglia in A.D. 876, 877 and 879-880 respectively. One of the most successful attempts at co-ordination concerns the southern boundary of the Pietish kingdom. The three conceptions—the historical conception derived from Bede and other historical sources, the archaeological conception derived from the distribution of symbol stones and the linguistic conception derived from the occurrence of the place-name element *pest*—come together with remarkable precision to demonstrate that the line of the Antonine Wall was the southern boundary of the political conception which is the kingdom of the historical Picts (see above, pp. 70-72, and Maps 4 and 5). In doing so they also demonstrate that the so-called Picts of Galloway are an illusion born of mistaken assumptions and unsound conclusions.

The fact that the three conceptions do not coincide over the whole of Pictland provides an example of the fact that there is no necessary correlation between historical, archaeological and linguistic conceptions. From historical sources we know that the Pietish kingdom stretched northwards from the Antonine Wall to include Orkney and Shetland, and the archaeological conception represented by the distribution of symbol stones (see Map 5) coincides closely with this historical and political conception. It is the linguistic conception represented by the distribution of the place-name element *pest* (see Map 4) that strikes the apparently discordant note. It has been suggested that the distribution of *pest*-names outlines a linguistic province predominantly Gallo-Brittonic and that the area to the north of it was predominantly non-Celtic and non-Indo-European (see above, pp. 70-71). This is reasonable as far as it goes, for no political or cul-

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tural conceptions are involved in this linguistic conclusion, but the picture would be clearer if we could bring it into chronological focus. From historical sources also we are able to divide Pictland into two political provinces represented by the 'northern Picts' and the 'southern Picts' (see above, p. 79), and it has been suggested that these two political provinces may find a cultural echo in two archaeological provinces that may be inferred from the comparative frequency of certain details of design and execution in the symbol stones of the two areas. But the observed differences may reflect no more than chronological delays in the spread of archaeological features, and in any case it should be remembered that the impression of uniformity conveyed by the symbol stones as a series reflects a broad cultural unity which must be associated with the political unity of the historical Pictish kingdom. No linguistic reflection of the 'northern' and 'southern' Picts has yet been recognized, and neither linguistic nor archaeological associations have been recognized for the seven later provinces of Pictland listed in historical sources. On the other hand it has been noted that the pre-Pictish archaeological conception of a broch area coincides in a remarkable way with the northern and predominantly non-Celtic linguistic province. There are many puzzling facets of the problem of the Picts, but it is useless to propose further associations at this stage, for mistaken associations are as harmful, or nearly as harmful, to effective co-ordination as are impossible equations.

It is only when conceptions can be associated, as distinct from when conceptions merely coincide, that they can be effectively co-ordinated. Conceptions that come together chronologically and territorially present the most straightforward opportunities for co-ordination, but

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conceptions not coincident in time and space can sometimes be brought into association with each other. Sometimes the evidence necessary for the formulation of a conception seems to be missing, sometimes it seems to be missing when in fact it is present but unrecognized, and sometimes it is misinterpreted. In these cases the different kinds of evidence and the corresponding conceptions may appear to be in conflict, but, as emphasized above, it will be an apparent conflict only and it will disappear when the evidence is correctly interpreted, when the proper assumptions are made or when the right key to co-ordination is found. Historical evidence can be misleading, as distinct from misinterpreted, but even this does not produce a true conflict of evidence, for a correct interpretation will recognize misleading evidence for what it is and so remove the apparent conflict produced by it. Archaeological and linguistic evidence, of course, cannot be misleading in themselves, though misinterpretation may produce misleading conclusions.

Examples of missing, apparently missing and apparently conflicting evidence are common. Students of the Anglo-Saxon settlement have long been puzzled by anomalies such as the lack of archaeological evidence for early settlement in Essex, where *-ingas* names are numerous, and by the lack of archaic place-names in Cambridgeshire and other areas where the archaeological evidence points to early settlement. Place-names throw a flood of light on the Danish settlement in England, but the archaeological reflection of this great movement is curiously faint—an occasional axe-head and one dubious cemetery is the only archaeological evidence that we can bring into association with it. Similarly the settlements of the Scots in Argyll, a movement which created the kingdom of Dalriada and

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ultimately the kingdom of Scotland, is well enough represented historically and linguistically but, apart from a fortress still in existence at Dunadd and the site of another at Dunollie, the archaeological record is faint and uncertain. As for the historical Picts, apart from their symbol stones, the difficulty of providing them with an archaeological context is notorious. Similarly the migration of Britons to Brittany is well enough attested by history, by language, by place-names and even, it is claimed, by physical anthropology, but the archaeological record of the movement eludes us.

Apparent anomalies such as those listed above may be explained by one or more of several possibilities, and these should always be kept in mind. The accident of discovery may operate against archaeological evidence like pagan burials coming within the limited range of our information; archaeological evidence may be destroyed by agricultural and industrial operations or buried beneath later towns and villages; and linguistic evidence may be swept away by equivalent developments in the field of language. Then, again, the evidence may be in front of us but not recognized, either because it appears in a form not foreseen or because our techniques of identification are inadequate for the task of isolating it. The Danes, for example, may have adopted Christian practices very soon after their arrival in England, and if they did we shall be disappointed if we believe that hundreds of pagan Danish burials still await discovery and wrong if we assume that they have been destroyed by a thousand years of agriculture. As for the linguistic evidence of the Anglo-Saxon settlement, we cannot claim to be able to recognize all place-names indicative of early settlement, and when we know more about archaic formations it may be that some

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of the apparent anomalies will disappear. Then there is always the possibility that what is clearly reflected in one conception may find no answering echo in another, or may be too faint to convey any coherent impression to the student. There seems to have been, for example, a considerable amount of inter-tribal migration and population movement among the Anglo-Saxon peoples: place-names suggest communities of Kentishmen (*Cantware*) at Canterton in Hampshire and Conderton in Worcestershire; East Saxons at Exton (*East Seaxnaton*) in Hampshire; other Saxons (*Seaxe*) in Anglican areas at Saxton in Cambridgeshire, Saxton in the West Riding of Yorkshire, Saxham in Suffolk and Saxondale in Nottinghamshire; Angles (*Engle*) in Saxon areas at Englebourne in Devon and Englefield in Berkshire; off-shoots of the *Wixan* in Worcestershire (Whitsun Brook) and in Middlesex (Uxbridge, Uxendon and Waxlow); and off-shoots of the *Hwicce* in Northamptonshire (Whiston), Staffordshire (Wichnor), etc. Several other indications of population movements have been noted in place-names, but the point is that these movements are not reflected in the political divisions of the Anglo-Saxon peoples recorded by Bede and other historical sources and they have not yet been recognized either in archaeological evidence or in the phonological evidence of place-names. They have failed to make any recognizable impression in any of these conceptions. If we had more detailed information about them it might throw light on dialectal peculiarities and it might resolve some of the complexities and remove some of the apparent anomalies in Anglo-Saxon archaeology.

An example of evidence that is misleading and an example of evidence that has been misinterpreted may both be quoted from a recent discussion on the date of

the Scandinavian settlement in Orkney. The evidence of history in the form of reliable annals of England, Ireland, Wales and the Continent indicates that Scandinavian raids on the British Isles began in the last decade of the eighth century, and chronological conclusions from archaeological material recovered from pagan Scandinavian graves in the islands suggest that the settlement began about A.D. 800. The two kinds of evidence produce conclusions that are in close harmony with each other. But the opinion of most philologists is that the settlement began long before A.D. 800, as early as A.D. 700 or even before A.D. 600, and they base their conclusions to a great extent on the occurrence of the place-name elements *vin* and *beimr*, which are thought to have become obsolete before the Viking Age. The evidence of the sagas, on the other hand, relates the settlement firmly to the battle of Hafsrfjord, which means that it must have taken place at a date not far removed from A.D. 900. Now a re-examination of the place-name evidence has shown that the elements *vin* and *beimr* will not support the chronological conclusion imposed upon them. The evidence of place-names has in fact been misinterpreted, and when the misinterpretations are corrected the evidence falls into line and brings support to the chronological conclusions based on the annals and on the archaeological evidence. This leaves the sagas standing alone and in apparent conflict. Leaving aside the wider issues of how far the historical evidence in the sagas can be trusted, it is clear that the chronological evidence here presented by them is misleading and must be disregarded.

It might be claimed that a similar argument would allow archaeologists to discard the early annals of the Anglo-Saxon Chronicle (see above p. 95), but it would not be a similar argument. The early part of the Anglo-Saxon

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Chronicle must be treated with critical reserve, as must all historical sources, and a correct interpretation requires some modification of the apparently specific statements in it, but it cannot be entirely discarded until there is some reason to believe that its nature makes it suspect as a historical source and until its testimony is contradicted with the same precision and unanimity as the testimony of the sagas is contradicted on the chronology of the Scandinavian settlement of Orkney.

What most requires emphasis is the extreme complexity of the problems we are trying to solve and of the pictures we are trying to recapture. All human activity in its several dimensions is inextricably complicated, and each of our conceptions offers at best only a faint reflection of one aspect of it. Simple solutions are to be suspected, and it should be remembered that any picture of the past recaptured by our inadequate techniques from the fragmentary evidence available to us cannot be more than a rough approximation to the truth, a fleeting glimpse of conditions and developments to a great extent outside the range of recovery.

IX

CONCLUSION

THIS book is about problems and difficulties, and for that reason it may seem both depressing in approach and didactic in character. Perhaps it is inevitable. The subject when treated at length hardly lends itself to lively and light-hearted writing. It is difficult to raise problem after problem without building up an atmosphere of gloom or to try to point out the way without seeming to preach. If, instead of dealing with problems and difficulties, we had recounted the achievements of co-ordination, this book might have been more like a triumphal progress and less like an excursion into the vale of shadows. But problems can be fascinating, perhaps especially when they cannot be solved, and the author confesses that, despite the burden of writing about them, the problems fascinate him even more at the end of this survey than they did at the beginning. Only the compelling need to return to the problems themselves prevents him from beginning this survey all over again. From which it will be understood that the comments and suggestions offered above are in-

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tended as contributions rather than as definite conclusions and final solutions.

It has not been central to our theme to emphasize the importance of effective co-ordination, but it needs no emphasis, for it is reflected on almost every page. The bringing together of historical, archaeological and linguistic evidence has already produced brilliant results, for each can offer information ill preserved or not preserved at all in the other two, and the co-ordination of reliable conclusions from each allows syntheses fuller and more revealing than any that could be reached by a single approach. The full impact of these advantages falls on the Dark Ages, for in this period the three kinds of evidence, each sadly deficient as a sole guide, have something like equality of status. It is the author's personal *credo* that the greatest advances in the illumination of this period will come in the future from more detailed and more reliable co-ordination, with the gradual evolution of more accurate and more comprehensive syntheses. Other approaches, not included in this survey, must not be forgotten, but there can be no doubt that history, archaeology and language are the three main avenues open to us.

In stressing the importance of co-ordination, however, one must not overlook the importance of specialization. The day of the *dilettante* is over, and without specialization and increasing specialization there is no hope for scholarship. Specialization finds complete and overwhelming justification in the results it has produced, and the only point that here calls for emphasis is that there is no conflict between specialization and co-ordination. A true specialist is a scholar with a breadth of vision allied to a mastery of detail, not one whose eyes are so glued to

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detail that he sees nothing of the background or of the relationship that exists between his own work and that of other scholars. Effective co-ordination can spring only from the most specialized of specialization, and at the same time it makes a fundamental contribution to the work of specialists by opening new possibilities to them and by redefining old questions with new precision.

There is no conflict between co-ordination and specialization, but there is a problem and one that has not yet been squarely faced in this book. It has been repeatedly urged that co-ordination can proceed effectively only from a thorough understanding of the nature of the different kinds of evidence involved and from an equally thorough appreciation of the techniques used and of the difficulties encountered in its collection and its interpretation. It follows that if one wishes to co-ordinate three different kinds of evidence one must become a specialist in each of three separate fields. It is not enough to rely on exchanges of question and answer, it is not enough to rely on opinions extracted from other specialists, it is not enough to rely on a general familiarity with the technical problems involved. It is necessary to practise in each field, as a historian tackling historical problems in the field of history, as an archaeologist tackling archaeological problems in the field of archaeology and as a philologist tackling linguistic problems in the field of place-names. It is necessary to become a specialist in the separate fields, accepting the standards and measuring up to the criticism of other specialists in each. It is not the attitude of the other specialists that raises a bar to ambitions of this kind, for as a rule they welcome any student who is prepared to undertake serious work and to submit himself to the standards which they themselves observe. The chief

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obstacle arises from the fact that there are only twenty-four hours in a day.

To those who argue that co-ordination is the enemy of specialization the answer is a blunt denial. But there is no convincing answer to those who argue that no single scholar today can become a specialist in more than one field of study. They may be right. The past offers notable examples of scholars who have met the challenge and have made important contributions to knowledge in separate fields, but each year success becomes more difficult to achieve. It might be possible to bring together a small group of specialists, each primarily a specialist in one field, but all bound together by a common interest in other fields and by a shared practical experience in them. There would be many difficulties of organization and administration, not least of finance; and it should also be borne in mind that the best scholars are individualists strongly determined to follow their own interests and usually unwilling to confine their researches within the limits of a pattern imposed from above. Despite the attractions of organized co-ordination, it looks as if we shall have to depend upon individual effort for some years to come.

Much can be done to bring an admittedly difficult programme within the bounds of possibility. One can define one's objectives, limit one's linguistic contributions to the field of place-names, concentrate on Old English and Old Norse and refuse to be drawn into the deeper mysteries of Celtic, and, while reading occasional books about the Bronze Age, strenuously resist being drawn into active archaeological participation in the study of any period outside the Dark Ages. It is not so easy as it sounds, for background knowledge is an important part of the

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equipment of any specialist. But it ought to be possible, in theory at any rate, for every historian, every archaeologist and every philologist must be content to specialize in a comparatively small section of his general field, and perhaps the task before the scholar who wishes to specialize in three fields is simply that of arranging his studies on a cross-section of his interests. Or perhaps that is just wishful thinking.

However one looks at the problem, the hazards are at least as great as the attractions. One can close one's eyes to the hazards and attempt to justify decisions by basing them on a set of beliefs but, as was written elsewhere in this book, 'a belief that a proposition is true is only indirect and often not very convincing evidence of its truth'. It would certainly be unkind to encourage young scholars to advance along a path which, though the journey is exciting and the goal attractive, may very well lead them to defeat and despair. They should consider well before they embark on such a journey, and no one will blame them if they turn aside to less hazardous pastures. Perhaps there will always be a few, blind or enthusiastic or both, who will refuse to count the cost.

MAPS



1. Distribution of certain early Place-name Elements (*-ing* and *-ingham*). The Chalk, Oolite and Lower Greensand Escarpments are shown.

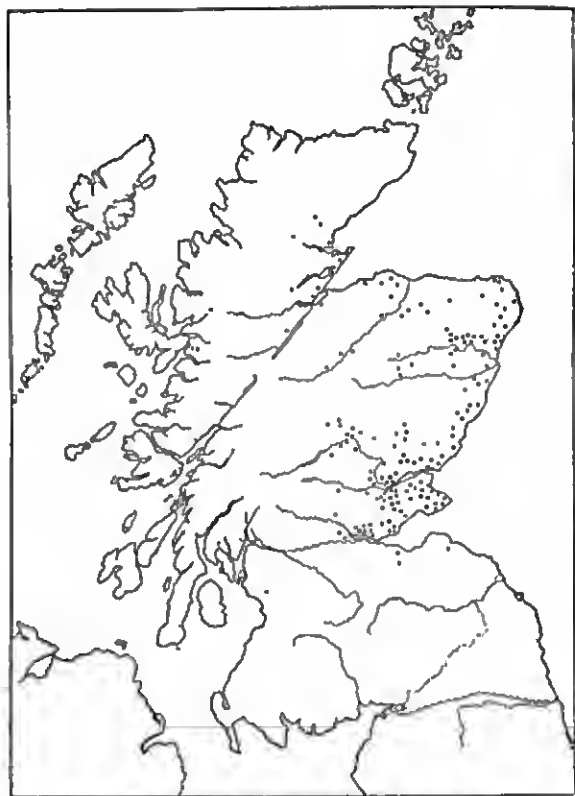


2. Distribution of Anglo-Saxon Burial Places. • Cemeteries, ° Single burials. The Chalk, Oolite and Lower Greensand Escarpments are shown.

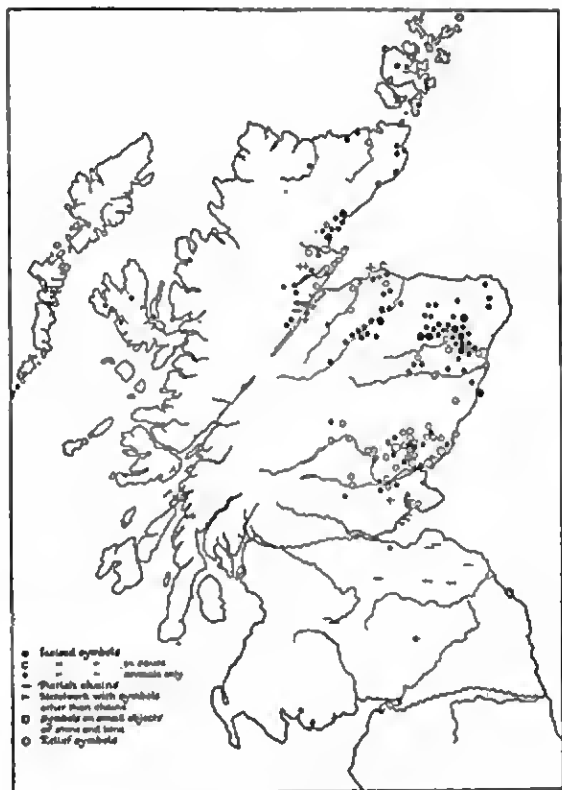




3. Place-names in *-by* existing in 1086.



4. Distribution of the Place-name Element *petr.*



5. Distribution of Pictish Symbols.



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